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REVIEW

Industrialization, Ideologies, and Social Structure *Reinhard Bendix*Demographic Data on China *Lawrence Krader and John S. Aird*

Socio-Economic Rank in Urban Research
Bernard Farber and John C. Osoinach

The Occupational Category in Research Edward GrossSampling a Metropolitan Community *Harry Sharp and Allan Feldt*Predicting a Community Decision *Robert C. Hanson*Equilibrium, Deviance, and Control *Theodore M. Mills*Crime, Age, and Employment *Daniel Glaser and Kent Rice*

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INDUSTRIALIZATION, IDEOLOGIES, AND SOCIAL STRUCTURE *

REINHARD BENDIX

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In the course of European industrialization ideologies of management have become concerned increasingly with the attitudes of workers. The managerial appeals of Anglo-American and of Russian civilization have differed, however, in terms of whether or not they presuppose the possibility of self-reliance and good faith. The study of such ideologies provides a clue to the changing class relations of industrial societies and, therefore, suggests that the analysis of cultural data is an important dimension of sociological research. To illustrate this point the paper shows how the basic difference between the managerial ideologies in England and Russia may be related to structural differences in bureaucratization.

SINCE World War II American social scientists have become preoccupied with the industrialization of underdeveloped areas. Considering the recent history of our disciplines, this is a relatively novel undertaking insofar as it involves the study of social change in complex social structures on a comparative basis. One approach to such a study consists in the selection of a social problem encountered in several societies but resolved differently in each. In a recent publication I used this approach by examining the authority relationship between employers and workers and the ideologies of management which justify that authority.¹ The present paper considers some implications of this analysis.

The first part of this essay summarizes the changes of ideology that have occurred in Anglo-American and in Russian civilization

over a two-hundred year period. The second part deals with the historical significance of ideologies of management, and the third part with the theoretical implications of a study that treats such ideologies as an index of social structure. In the fourth part I turn to the problem of bureaucratization and to the difference between totalitarian and non-totalitarian forms of subordination in industry.

CHANGES IN IDEOLOGY

At the inception of industrialization in England an ideology of traditionalism prevailed; John Stuart Mill called it the "theory of dependence." According to this view the laboring poor are children, who must be governed, who should not be allowed to think for themselves, who must perform their assigned tasks obediently and with alacrity, who must show deference to their superiors, and who—if they only conduct themselves virtuously—will be protected by their betters against the vicissitudes of life. This interpretation of authority is self-confirming and

* Revised text of the MacIver Lecture delivered before the District of Columbia Sociological Society, February 3, 1959.

¹ Reinhard Bendix, *Work and Authority in Industry*, New York: Wiley, 1956.

self-serving.² But it sets up the presumption that the dependence of the poor and the responsibility of the rich are the valid moral rules of the social order. In the course of industrial development these ideas were gradually modified. As the responsibility of the rich was increasingly rejected by the advocates of laissez-faire, the dependence of the poor was turned from an inevitable into a self-imposed fate. As it was "demonstrated" that the rich cannot care for the poor without decreasing the national wealth, it was also asserted that by abstinence and exertion the poor can better their lot. The same virtues which in the 18th century were extolled so that the lowly will not aspire above their station were praised by the middle of the 19th century because they enable a man to raise himself by his own efforts.

In England, and even more in America, this praise of effort led toward the end of the 19th century to an apotheosis of the struggle for existence. The militant language of an ethics of the jungle was applied to the relations between employers and workers. Riches and poverty merely reflect differences of ability and effort. The employer's success is evidence of his fitness for survival, and as such justifies his absolute authority over the enterprise. This assertion of authority has a clear-cut meaning only as long as most managerial functions are in the hands of one man. The idea becomes ambiguous as the use of expertise in the management of enterprises increases and the managerial function becomes subdivided and specialized. Yet the idea of the employer's absolute authority over his enterprise coincided with the "scientific management" movement which sought to give him expert advice on what to do with that authority. It may be suggested, therefore, that the doctrines of Social Darwinism gradually lost their appeal, in part because changes in industrial organization gave rise to a changing imagery of men in industry. From the Gilded Age to the 1920s, workers and managers were self-evident failures or

successes in a struggle for survival, in which they were the recalcitrant objects or the exasperated originators of managerial commands. Today they have become individuals-in-groups whose skills must be improved and allocated systematically and whose productivity must be maximized by appropriate attention to their psychological makeup. Thus, over the past two hundred years, managerial ideologies in Anglo-American civilization have changed from the "theory of dependence" to laissez-faire, to Social Darwinism, and finally to the "human relations" approach.

In the Russian development we also find the assertion of paternal authority and of child-like dependence, and in much the same terms as in England. But in Russia this ideology of traditionalism was a very different thing from what it was in England because of the Tsar's assertion of supreme authority over all the people. This authority remained intact regardless of how many privileges the Tsar granted to the landlords and regardless of how rarely he interfered in fact with the use and abuse of these privileges. Ideologically the Tsar maintained his preeminence through repeated assertions concerning his paternal care and responsibility for all of "his" people. Through repeated petitions and sporadic revolts the people used this Tsarist claim in order to obtain redress for their grievances against landlords and employers. Finally, because of the early centralization of authority under the Muscovite rulers, the whole distribution of wealth and rank among the aristocracy turned upon the competition for favors at the Court and hence reinforced the Tsar's supremacy.³

During the second half of the 19th century this pattern of Tsarist autocracy had far-reaching consequences. The dislocations in-

³ In Russia the landed aristocracy never succeeded in making itself the unavoidable intermediary between the ruler and the people in contrast with Western Europe, where the ruler's administrative and juridical authority in effect ended at the boundaries of the estate, though this contrast merely states the end-result of protracted struggles over the division of authority. Cf. Max Weber, *Wirtschaft und Gesellschaft*, Tuebingen: Mohr, 1925, II, Chapter 7 and esp. pp. 720-723, and Otto Hintze, "Weltgeschichtliche Bedingungen der Repräsentativverfassung," *Historische Zeitschrift*, 143 (1931), pp. 1-47.

² The laboring poor are asked to prove their virtue by their obedience, but they are also told that their dependence results from a natural inferiority. Similarly, the ruling classes are said to be responsible for the deserving poor, and if they do not meet this responsibility, it is only, they say, because the poor who suffer are not deserving.

cident to the emancipation of the serfs (1861) and the development of industry brought in their train assertions of absolute authority by the employers, efforts of the workers to organize themselves, and sporadic attempts of the government to regulate the relationship between them. Although ostensibly acting on an equitable basis, the government in fact supported the employers against the workers. Much of this is again broadly familiar from the English experience; but Russia's historical legacies prevented the shift in ideology which has been described for England. As long as Tsarist autocracy remained intact neither the rejection of responsibility by the Tsar and the ruling strata nor the demand for the self-dependence of the workers developed. Instead, the Tsar and his officials continued to espouse the ideology of traditionalism. Quite consistently, Tsarist officials sought to superintend both employers and workers in order to mitigate or suppress the struggles between them. That is, the officials aided and curbed the employers' exercise of authority as well as the workers' efforts to formulate grievances and organize protest movements.

Tsarist autocracy was overthrown in the Russian revolutions of 1905 and 1917. Although vast differences were brought about by the revolution, the managerial ideology of Tsarism lived on in a modified form. In theory, Tsarist officials had regarded employers and workers as equally subject to the will of the Tsar; loyal submission to that will was the mark of good citizenship. In theory, Lenin believed that all workers were equal participants in the management of industry and government; their loyal submission to the Communist party represented their best interest and expressed their sovereign will. The logic of Lenin's as of the Tsarist position is that under a sovereign authority the same person or organization can and should perform both subordinate and superordinate functions. For example, Soviet labor unions approach the ideal of workers' control of industry when they are called upon to participate in the management of industry. But they also function in a managerial capacity when they inculcate labor discipline among their members under the authoritative direction of the Communist Party.

Ideologically this position is defended on

the ground that the party represents the historical interests of the proletariat against the short-run interests of individuals and factions. In this orientation one can still see survivals of Tsarist autocracy since all wisdom and responsibility reside in a small group or indeed in one man who, like the Tsar, knows better than private persons what is the good of all, and cannot but wish the well-being of the people. But there is also an important difference. The leaders of the Russian revolution were faced with the task of developing self-discipline and initiative among workers if a suitable industrial workforce was to become available.⁴ They proceeded to inculcate these qualities by the direct or indirect subordination of everyone to the discipline of the Communist party. This policy continued the Tsarist tradition by making all matters the object of organizational manipulation rather than of personal striving; but it also represented a break with the past in that it was no longer restricted to personal submission. I shall have more to say on this subject in the fourth part of this paper.

HISTORICAL SIGNIFICANCE OF IDEOLOGICAL CHANGE

What are the historical implications of this analysis of managerial ideologies? Ruling groups everywhere, including the rulers of developing industrial societies, justify their good fortune as well as the ill fortune of those subject to their authority. Their self-serving arguments may not appear as a promising field of research; in fact, the whole development of industrialization has been accompanied by an intellectual rejection of such ideologies as unworthy of consideration. Yet the fact is that all industrialization involves the organization of enterprises in which a few command and many obey; and the ideas developed by the few and the many, I believe, may be considered a symptom of changing class relations and hence as a clue to an understanding of industrial societies.⁵

⁴ Lenin's statement that "the Russian is a bad worker" and his advocacy of the Taylor system and of electrification as the road to socialism are indicative of the fact that the problems of complex industrial organizations came to the fore at once.

⁵ See Bendix, *Work and Authority*, pp. xvii-xviii, 1-2.

Historically, ideologies of management became significant in the transition from a pre-industrial to an industrial society. The authority exercised by employers was recognized as distinct from the authority of government. This was a novel experience even in Western Europe where there was precedent for such autonomy in other institutions, because the industrial entrepreneurs were "new men" rather than a ruling class buttressed by tradition. This was also the period during which the discipline of sociology originated. Under the impact of the French revolution society came to be conceived in terms of forces that are independent from, as well as antagonistic to, the formal institutions of the body politic. Some early elaborations of this key idea enable us to see the historical significance of ideologies of management.

The authority of employers rests on the contractual acquisition of property, which the 18th century philosophers made the conceptual basis of the social order. In Rousseau's view that order can be and ought to be based on a general will which presupposes that the individual acts for the whole community. In such a society, as George Herbert Mead has pointed out, "... the citizen can give laws only to the extent that his volitions are an expression of the rights which he recognizes in others, ... [and] which the others recognize in him. ..."⁶ This approach provides a model for a society based on consent so that the power of rule-making is exercised by all and for all. This foundation of society upon a "general will" was directly related to the institution of property. As Mead has stated,

If one wills to possess that which is his own so that he has absolute control over it as property, he does so on the assumption that everyone else will possess his own property and exercise absolute control over it. That is, the individual wills his control over his property only in so far as he wills the same sort of control for everyone else over property.⁷

Thus, the idea of a reciprocal recognition of rights specifically presupposed the equality of citizens as property-owners.

⁶ George Herbert Mead, *Movements of Thought in the Nineteenth Century*, Chicago: University of Chicago Press, 1936, p. 21.

⁷ *Ibid.*, p. 17.

This implication gave pause to some 18th and 19th century philosophers. They noted that the reciprocity of rights among property owners based on freedom of contract does not apply to the relations between employers and workers. As early as 1807 the German philosopher Hegel formulated the problematic nature of this relationship in a manner which anticipates the modern psychology of the self, just as Rousseau's "general will" anticipates the sociological analysis of interaction. Hegel maintains that men come to a recognition of themselves through a process whereby each accepts the self-recognition of the other and is in turn accepted by him. That is, each man's sense of identity depends upon his acceptance of the identity of others and upon their acceptance of himself. In Hegel's view this reciprocity is lacking in the relation between master and servant. The master does not act towards himself as he acts towards the servant; and the servant does not do towards others what his servitude makes him do against himself. In this way the mutuality of recognition is destroyed and the relations between master and servant become one-sided and unequal.⁸

In Western Europe this inequality of the employment-relationship coincided with the ideological and institutional decline of traditional subordination. Yet while the old justifications of subordination crumbled and new aspirations were awakened among the masses of the people, their experience of inequality continued. According to Tocqueville this problem had a differential impact upon masters and servants. In the secret persuasion of his mind the master continues to think of himself as superior; but he no longer recognizes any paternal responsibilities toward the servant. Still, he wants his servants to be content with their servile condition. In effect, the master wishes to enjoy the age-old privileges without acknowledging their concomitant obligations; and the servant rebels against his subordination, which is no longer

⁸ Georg Friedrich Wilhelm Hegel, *Phänomenologie des Geistes*, Leipzig: Felix Mainer, 1928, pp. 143, 147. My paraphrasing attempts to convey Hegel's meaning without use of his language. The relevant passages are readily accessible in C. J. Friedrich, editor, *The Philosophy of Hegel*, New York: Modern Library, 1953, pp. 399-410.

a divine obligation and is not yet perceived as a contractual obligation.

Then it is that [in] the dwelling of every citizen . . . a secret and internal warfare is going on between powers ever rivals and suspicious of each other: the master is ill-natured and weak, the servant ill-natured and intractable; the one constantly attempts to evade by unfair restrictions his obligation to protect and to remunerate, the other his obligation to obey. The reins of domestic government dangle between them, to be snatched at by one or the other. The lines that divide authority from oppression, liberty from license, and right from might are to their eyes so jumbled together and confused that no one knows exactly what he is or what he may be or what he ought to be. Such a condition is not democracy, but revolution.⁹

In the 19th century men like Hegel, Tocqueville, and Lorenz von Stein pointed out that the spread of equalitarian ideas was causing a transition in the relations between masters and servants. This transition may be called a crisis of aspirations. In Tocqueville's words the servants "consent to serve and they blush to obey. . . . [They] rebel in their hearts against a subordination to which they have subjected themselves. . . . They are inclined to consider him who orders them as an unjust usurper of their own rights."¹⁰ As a consequence most European countries witnessed the rise of a "fourth estate" which struggled against existing legal liabilities and for basic civil rights, above all the right to suffrage. In a parliamentary debate on Chartism, Disraeli remarked that this struggle was invested with a degree of sentiment usually absent from merely economic or political contests. To the extent that such complex movements can be characterized by a common denominator this sentiment referred, I think, to the workers' quest for a public recognition of their equal status as citizens.¹¹ Where this and other civil rights became accepted, such recognition compensated for the continued social and economic subordination

of the workers and thus assuaged the crisis of aspirations. Moreover, the political utilization of these civil rights could lead to a recognition of basic social rights which today is embodied in the institutions of social welfare characteristic of many Western democracies.¹² The initial crisis of aspirations continued, on the other hand, where civil rights were rejected or where their acceptance was postponed for too long, leading either to an eventual revolutionary upheaval as in Tsarist Russia, or to a more or less damaging exacerbation of class-relations as in Italy and France.

My hypothesis is that the break with the traditional subordination of the people gave rise to a generic problem of many industrial societies.¹³ The question of 19th century Europe concerned the terms on which a society undergoing industrialization will incorporate its newly recruited industrial work force within the economic and political community of the nation. Ideologies of management are significant because they contribute to each country's answer to this question. In England the workers were invited to become their own masters, if they did not wish to obey; in Russia they were told that their subordination was less onerous than it seemed, because their own superiors were also servants of the almighty Tsar.

THEORETICAL SIGNIFICANCE OF IDEOLOGIES

What are the theoretical implications of this approach? Ideologies of management may be considered indexes of the flexibility or rigidity with which the dominant groups in the two countries were prepared to meet the challenge from below. This "preparedness" or collective tendency to act is analogous to the concept of character-structure in the individual: it may be defined as an "inner capacity" for recreating similar lines of action under more or less identical condi-

⁹ Alexis de Tocqueville, *Democracy in America*, New York: Vintage Books, 1945, II, p. 195. Some phrases in the preceding paragraph are also taken from this chapter of Tocqueville's work.

¹⁰ *Ibid.*

¹¹ See Bendix, *Work and Authority*, pp. 34-46, 150-162. I deal with this aspect in more detail in a forthcoming monograph on *Class Relations and European Industrialization*.

¹² For a perceptive analysis of this development see T. H. Marshall, *Citizenship and Social Class*, Cambridge: At the University Press, 1950, Chapter 1. The statement in the text refers specifically to England. Social rights have been instituted in other ways, sometimes in order to withhold the establishment of civil rights as in Imperial Germany.

¹³ An expanded statement of this point will be found in my article "A Study of Managerial Ideologies," *Economic Development and Cultural Change*, 5 (January, 1957), pp. 118-128.

tions.¹⁴ The ideologies of management, which reflect this "inner capacity," naturally provoke new challenges and these in turn lead to new managerial responses, so that at the societal level there is a replication of the action-reaction process so typical of interaction among individuals.

An analysis of this process must deal with those explicitly formulated ideas that are as close as possible to the collective experience of employers and workers. This social philosophizing of and for the ordinary man as a participant occurs at a level somewhere between his attitudes as an individual and the sophisticated formulations of the social theorist. Such philosophizing is exemplified by what Andrew Ure wrote in his *Philosophy of Manufactures* or by what the publicity-men for General Motors say in their pamphlet *Man to Man on the Job*. However, the serious analysis of such documents is at variance with the prevailing tendency to dismiss them as obviously biased and hence unworthy of consideration on their own terms. Marx, it may be recalled, reserved some of his choicest invective for his characterization of Ure's book, and in this respect Marx was a forerunner of the intellectuals born in the 1850s and 1860s. Freud, Durkheim, Pareto, and others shared with Marx the search for some underlying principle or force that could explain the manifest beliefs and actions making up the external record of individual and collective behavior.¹⁵ Many writers of this generation were less interested in what a man said, than in why he said it. Accordingly, ideologies of management might be dismissed because they merely express a class-interest,

or because they do not reveal the *real* attitudes of the employers, or because they disguise *actual* exploitative practices, or because all this talk tells us nothing about man's behavior or about his personality structure. These various objections have in common an intellectual preoccupation with covert forces that can explain the manifest content of the social world.

Modern social science owes to this intellectual tradition many important insights, but also many of its aberrations. Where the phenomena of the social world are treated merely as the reflection of "hidden forces," speculation easily becomes uncontrolled with the result that observable evidence is dismissed from consideration as being "irrelevant" or "uninteresting" on theoretical grounds. The difficulty is familiar in Marx's theory of history which encouraged him to treat whole series of facts as epiphenomena, such as the "false consciousness" of the workers that was bound to be superseded in the course of history. Similarly, the Freudian approach tends to devalue a behavioristic study of social life because it deals with the appearance rather than the underlying motivations of social action. Again, the use of organic analogies in the study of society treats all actions as dependent adjustments to other actions (or environmental conditions); consequently this approach devalues all deliberate and all innovative activity, since upon analysis such activity will be revealed as yet another dependent adjustment. In in-expert hands all of these approaches lead to a cavalier construction of the evidence which can always be more easily imputed to the "underlying determinants" than analyzed in detail on its own ground.

Yet human experience occurs at this phenomenological level—and the study of ideologies of management illustrates that it can also provide an approach to our understanding of the social structure.¹⁶ The managerial

¹⁴ The quoted phrase occurs in Burckhardt's definition of the objective of culture-history, which "goes to the heart of past mankind [because] it declares what mankind *was*, *wanted*, *thought*, *perceived*, and *was able to do*. In this way culture history deals with what is constant, and in the end this constant appears greater and more important than the momentary, a quality appears to be greater and more instructive than an action. For actions are only the individual expressions of a certain inner capacity, which is always able to recreate these same actions. Goals and presuppositions are, therefore, as important as events." See Jacob Burckhardt, *Griechische Kulturgeschichte*, Stuttgart: Kroener, 1952, Vol. I, p. 6.

¹⁵ Cf. H. Stuart Hughes, *Consciousness and Society*, New York: Knopf, 1958, which gives a perceptive analysis of this "generation."

¹⁶ By "ideologies" I do not refer to attitudes of the type that can be elicited in a questionnaire study, but to the "constant process of formulation and reformulation by which spokesmen identified with a social group seek to articulate what they sense to be its shared understandings." See *Work and Authority*, p. xxii. I call these articulations "ideologies" in the specific sense of "ideas considered in the context of group-action." All ideas may be analyzed from this viewpoint; hence I depart from

interpretations of the authority relationship in economic enterprises together with the workers' contrast-conception concerning their collective position in an emerging industrial society constitute a composite image of class relations which has changed over time and which also differs from country to country. This aspect of the changing social structure may be studied by examining each ideological position in terms of its logical corollaries as these relate to the authority of the employers and in a wider sense to the class position of employers and workers in the society. Where these corollaries create major problems for the complacent self-interest of the group, one may expect the development of tensions, and perhaps of change, ideologically and institutionally.¹⁷

Such ideologies, and this is a second level of analysis, are in part expedient rationalizations for the problems confronting the entrepreneur, and in part the result of historically cumulative response-patterns among social groups. In this way ideologies are formulated through the constant interplay between current contingencies and historical legacies. As Marx put it, "men make their own history," but they do so "under circumstances directly given and transmitted from the past." (Marxian dogmatism consistently sacrificed the first to the second part of this generalization.¹⁸ Accordingly, ideologies of

management can be explained only in part as rationalizations of self-interest; they also result from the legacy of institutions and ideas which is "adopted" by each generation much as a child "adopts" the grammar of his native language. Historical legacies are thus a part of the social structure: they should not be excluded from a discipline that focusses attention upon the persistence of group-structures. In the following section an attempt is made to show the link between historical legacies and the structure of industrial societies by relating ideologies of management to the bureaucratization of industry.

IDEOLOGIES, INDUSTRIAL BUREAUCRACY, AND TOTALITARIANISM

Since the 18th century Anglo-American and Russian civilizations have witnessed a growing managerial concern with the attitudes as well as the productivity of workers. It is possible to relate this change of ideology to a large number of the developments which comprise the transition from an early to a mature industrial society. The changing structure of industrial organizations was only one of these developments. Yet the bureaucratization of economic enterprises is of special importance for any attempt to "interpret the difference of fact and ideology between a totalitarian and nontotalitarian form of subordination in economic enterprises."¹⁹ Bureaucratization is also especially suitable for a comparative study of authority relations in industry, since it involves processes that are directly comparable in two such different civilizations as England and Russia. This choice of focus deliberately eschews a comprehensive theory of society in favor of selecting a problem which, if suitable for comparative analysis, will also lead to an analysis of social structures. For, if comparable groups in different societies confront and

the identification of "ideologies" with false or misleading ideas.

¹⁷ For example, at the turn of the century American employers asserted their absolute authority over the workers but this assertion lacked content until the bureaucratization of industry brought to the fore experts who worked out methods for the exercise of authority. Again, the Tsar's assertion of authority over all the people inadvertently encouraged the peasants to appeal to the Tsar for redress of grievances. This procedure is adapted from that used by Max Weber in his sociology of religion.

¹⁸ The sentence immediately following this quotation reads: "The tradition of all the dead generations weighs like a nightmare on the brain of the living." See Karl Marx, *The 18th Brumaire of Louis Bonaparte*, New York: International Publishers, n.d., p. 13. I do not accept this polemical exaggeration, since traditions are enabling as well as disabling, but the emphasis upon the impact of cultural tradition on current ideologies is more in line with the facts than the effort to explain the latter solely in terms of the problems the businessman encounters in his work. Such an interpretation leads to an elimination of ideological changes, and of differences

between ideologies, since all ideologies are in this sense responses to the strains endemic in modern society. Cf. Francis X. Sutton et al., *The American Business Creed*, Cambridge: Harvard University Press, 1956, *passim*, where the change of business ideologies over time is denied and where these ideologies are explained in exactly the same terms as nationalism and anti-capitalism. See also the comments of Leland Jenks, "Business Ideologies," *Explorations of Entrepreneurial History*, 10 (October, 1957), pp. 1-7.

¹⁹ *Work and Authority*, p. xx.

over time resolve a common problem, then a comparative analysis of their divergent resolutions will reveal the divergence of social structures in a process of change.²⁰

Problems of a systematic management of labor come to the fore where the increasing complexity of economic enterprises makes their operation more and more dependent upon an *ethic of work performance*. This ethic involves a degree of steady intensity of work, reasonable accuracy, and a compliance with general rules and specific orders that falls somewhere between blind obedience and unpredictable caprice. Where personal supervision is replaced by impersonal rules the efficiency of an organization will vary with the degree to which these attributes of work-performance are realized, and this realization is part of the on-going bureaucratization of economic enterprises. That is to say, management subjects the conditions of employment to an impersonal systematization, while the employees seek to modify the implementation of the rules as their personal interests and their commitment (or lack of commitment) to the goals of the organization dictate. As everyone knows, there is no more effective means of organizational sabotage than a letter-perfect compliance with all the rules and a consistent refusal of the employees to use their own judgment. "Beyond what commands can effect and supervision can control, beyond what incentives can induce and penalties prevent, there exists an exercise of discretion important even in relatively menial jobs, which managers of economic enterprises seek to enlist for the achievement of managerial ends."²¹ In the

literature on organizations this exercise of discretion by subordinates is known by a number of terms: Veblen called it the "withdrawal of efficiency;" Max Weber referred to it as the bureaucratic tendency towards secrecy; Herbert Simon might call it the "zone of non-acceptance." I have suggested the phrase "strategies of independence" so as to get away from the negative connotations of the other terms, since the exercise of discretion may serve to achieve, as well as to subvert, the goals of an organization.

Now, the great difference between totalitarian and nontotalitarian forms of subordination consists in the managerial handling of this generic attribute of all authority relations. The historical legacies of some Western countries have encouraged management to presuppose the existence of a common universe of discourse between superiors and subordinates, and this presupposition is related to the successful resolution of the crisis of aspirations. From the evangelism and the tough-minded laissez-faire approach of 18th century England to the latest refinement of the "human relations" approach, managerial appeals have been addressed to the good faith of subordinates in order to enlist their cooperation. Whether such good faith existed is less important than that such appeals were made, though it is probable that in England and the United States large masses of workers in one way or another accepted managerial authority as legitimate even if they were indifferent to, or rejected, the managerial appeals themselves.²² In Russia, on the other hand, historical legacies did *not* encourage management (under the Tsars) to presuppose the existence of a common universe of discourse between superiors and subordinates. From the time of Peter the Great to the period of rapid industrial growth in the last decades preceding World War I managerial appeals were addressed to the workers' duty of obedience towards all those in positions of authority. Whether or not the workers actually developed a sense of duty, the appeals presupposed that they had not. Accordingly, officials and managers did not

²⁰ Here again I am indebted to the work of Max Weber, although more to what he did in his own studies than to what he wrote about them in his methodology. See my forthcoming *Max Weber, An Intellectual Portrait*, New York: Doubleday, 1960, Chapter 8.

²¹ *Work and Authority*, p. 251. To avoid a possible misunderstanding I add that this assertion, which is elaborated in *ibid.*, pp. 244-251, is in my judgment compatible with the endeavor to put managerial decision-making on a more scientific basis. The substitution of machine methods for manual operations is obviously an on-going process that has greatly curtailed the areas of possible discretion, although machine methods also create new opportunities for discretionary judgments. But while these methods and organizational manipulations may curtail and reallocate the areas in which discretion

is possible or desired, and may in this way achieve greater efficiency, they cannot, I believe, eliminate discretion.

²² Cf. *Work and Authority*, pp. 248-249, for a fuller statement.

rely on the good faith among their subordinates, but attempted instead to eliminate the subordinates' strategies of independence.

This managerial refusal to accept the tacit evasion of rules and norms or the uncontrolled exercise of judgment is related to a specific type of bureaucratization which constitutes the fundamental principle of totalitarian government. In such a regime the will of the highest party authorities is absolute in the interest of their substantive objectives. The party may disregard not only all formal procedures by which laws are validated but also its own previous rulings; and where norms may be changed at a moment's notice, the rule of law is destroyed. Totalitarianism also does away with the principle of a single line of authority. Instead of relying on an enactment of laws and on the supervision of their execution from the top, totalitarian regimes use the hierarchy of the party in order to expedite and control at each step the execution of orders through the regular administrative channels. This may be seen as the major device by which such regimes seek to prevent officials from escaping inspection while compelling them to use their expertise in an intensified effort to implement the orders of the regime. A totalitarian government is based, therefore, on two interlocking hierarchies of authority. The work of every factory, of every governmental office, of every unit of the army or the secret police, as well as every cultural or social organization, is programmed, coordinated, and supervised by some agency of government. But it is also propagandized, expedited, criticized, spied upon, and incorporated in special campaigns by an agency of the totalitarian party, which is separately responsible to the higher party authorities.

The rationale of this principle of a double government can be stated within the framework of Max Weber's analysis of bureaucracy. An ideally functioning bureaucracy in his sense is the most efficient method of solving large-scale organizational tasks. But this is true only if these tasks involve a more or less stable orientation towards norms which seek to maintain the rule of law and to achieve an equitable administration of affairs. These conditions are absent where tasks are assigned by an omnipotent and revolutionary authority. Under the simulated combat con-

ditions of a totalitarian regime the norms that govern conduct do not stay put for any length of time, although each norm in turn will be the basis of an unremitting drive for prodigies of achievement. In response, subordinates will tend to use their devices of concealment for the sake of systematic, if tacit, strategies of independence. They will do so not only for reasons of convenience, but because the demands made upon them by the regime are "irrational" from the viewpoint of expert knowledge and systematic procedure.²³ The party, on the other hand, seeks to prevent the types of concealment that make such collective strategies possible by putting every worker and official under maximum pressure to utilize their expertise to the fullest extent. This is the rationale of a double hierarchy of government, which places a party functionary at the side of every work unit in order to prevent concealment and to apply pressure. The two hierarchies would be required, even if all key positions in government and industry were filled by party functionaries. For a functionary turned worker or official would still be responsible for "overfulfilling" the plan, while the new party functionary would still be charged with keeping that official under pressure and surveillance.²⁴

In this way totalitarianism replaces the old system of stratification by a new one based on criteria of activism and party orthodoxy. The ethic of work performance on which this regime relies is not the product of century-

²³ Hence they will do so even for the purpose of achieving the objectives of the party itself. Cf. Joseph Berliner, *Factory and Manager in the USSR*, Cambridge: Harvard University Press, 1957, which documents that the most successful Soviet managers use the systematic subversion of authoritative commands for the purpose of realizing the ends of these commands as well as for their personal convenience. This fact suggests that "good faith" can be inculcated in many ways, even by the systematic distrust of all subordinates, provided of course that the distrust has a higher rationale, such as the utopian and nationalist ideology of Russian Communism.

²⁴ A case study of totalitarianism in the context of industrial relations is contained in *Work and Authority*, Chapter 5. For a more generalized treatment of this approach to totalitarianism, cf. Bendix, "The Cultural and Political Setting of Economic Rationality in Western and in Eastern Europe," in the forthcoming Gregory Grossman, editor, *Economic Calculation and Organization in Eastern Europe*, Berkeley: University of California Press.

long growth as in the West, but of material incentives and of a political supervision that seeks to prevent evasion from below as well as from above. For example, the collective "bargaining" agreements of Soviet industry are in fact declarations of loyalty in which individuals and groups pledge themselves publicly to an overfulfillment of the plan, while the subsequent organization of public confessionals, the manipulation of status differences between activists and others, the principle of collective leadership, and further devices seek to maximize performance and prevent the "withdrawal of efficiency." The individual subordinate is surrounded almost literally. Aside from ordinary incentives he is controlled by his superior and by the party agitator who stands at the side of his superior; but he is also controlled "from below" in the sense that the social pressures of his peer group are manipulated by party agitators and their agents. This institutionalization of suspicion and the consequent elimination of privacy are justified on the ground that the party "represents" the masses, spearheads the drive for Russian industrialization, and leads the cause of world communism.

SUMMARY

The purpose of this paper is to state the case for a comparative analysis of social structures, which pays attention to the historical continuity of societies as well as to the concatenation of group structures and deliberate, self-interested action in the process of social change. In lieu of abstract considerations I have tried to make this case by analyzing some implications of ideologies of management in the course of industrialization.

The change of ideologies of management during the last two centuries in Anglo-American and in Russian civilization was similar in so far as it can be characterized as an increased managerial concern with the attitudes of workers that presumably account for their differential productivity. This overall similarity coincides, however, with a fundamental divergence. In Western civilization the authority relations between employers and workers remained a more or less autonomous realm of group activity even where the "human relations" approach has replaced

the earlier individualism. In Russia, the employment relationship has been subjected throughout to a superordinate authority which regulated the conduct of employers and workers and which could transform superiors into subordinates or (more rarely) subordinates into superiors, when governmental policies seemed to warrant such action.

This comparison of ideologies of management is significant for specific historical reasons in addition to the fact that authority relations in economic enterprises are a universal attribute of industrialization and hence lend themselves to a comparative analysis. Ideologies of management became significant when the equalitarianism of property owners, brought to the fore by the French revolution and by the legal codifications which followed, was contrasted with the inequality of the employment relationship. A heightened awareness of this inequality coincided with the decline of a traditional subordination of the lower classes and hence with a rise of aspirations for social and political as well as for legal equality. In England these demands for equal rights of citizenship on the part of the lower classes eventuated in a painful but peaceful reconstitution of class relations; in Russia, the same demands were rejected and finally led to the revolutions of 1905 and 1917.

The comparative study of ideologies of management is of theoretical as well as of historical interest. Such ideologies may be considered indexes of a readiness to act, which together with the ideological responses of other groups, can provide us with a clue to the class relations of a society. Ideologies, it is assumed, are an integral part of culture, which should be analyzed on its own terms as an index of the social structure, much as the neurotic symptoms of an individual are analyzed as an index of his personality. It is further assumed that such ideologies are expedient rationalizations of what are taken to be the material interests of a group, but that such rationalizations tend to be circumscribed by the historical legacies which are a part of a country's developing social structure.

Although ideologies of management can be treated as a clue to class relations, it is also worthwhile to relate them to other aspects

of the social structure. One such aspect, which is especially suitable for a comparison of totalitarian and non-totalitarian regimes, is the fact that all industrial enterprises undergo a process of bureaucratization and all bureaucracy involves the use of discretion in the execution of commands. Comparison between the Anglo-American and the Russian tradition reveals that in the two cases managerial appeals have differed in terms of whether or not they have presupposed the good faith of subordinates. Where that supposition has not been made, the drive for industrialization takes the specific form of a double hierarchy of government which is designed to apply maximum pressure on subordinates and to forestall their evasion of commands by supplementing executive with political controls at every point in the chain of command.

Both English and American and Russian industrialization have been marked by bureaucratization, and bureaucratization certainly threatens the development of initiative.²⁵ But the Soviet case also illustrates that this threat may provoke counter-measures. One might speak of an institutionalization of initiative in the totalitarian party and one can speculate that the dynamic drive of the Soviet regime might be jeopardized by too much relaxation of a Cold War which appears to justify that drive. This is, I submit, the new context in which the comparative study of ideologies of management will continue to be an intellectual challenge.

²⁵ Cf. Joseph Schumpeter, *Capitalism, Socialism and Democracy*, New York: Harper, 1950, where this theme is elaborated.

SOURCES OF DEMOGRAPHIC DATA ON MAINLAND CHINA *

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The 1953 census of Mainland China disclosed a population of 582.6 million. Demographic data from official sources seem to be reliable and useful despite defects in data collection and analysis, particularly, underenumeration of females and infants and distortion of the age-sex structure. Use of population registers for public security purposes has weakened the registration system. Data on vital rates are affected by underregistration. Urban growth data suffer from underregistration of rural migrants. Demographic field surveys fail to apply standard sampling procedures and controls. There is no evidence that demographic data are fabricated for propaganda purposes. In Peiping, policy and planning are based on these data. With appropriate adjustment, they may be used in demographic research on Mainland China.

THE Chinese Communists have thus far published very few data on the present population of Mainland China. Little or no information is available on population densities, internal migration, family size and composition, marital status, marriage and divorce rates, and the population of civil divisions smaller than provinces. There are some data on labor force composition, age and sex distribution, the size of major ethnic groups, the size of cities of 100,000 or over, and the growth of rural and urban sectors

of the population from 1949-1956. Recent publications have given the population by province for 1954 and figures for individual provinces for subsequent years. Occasional figures have been released on local and national vital rates, sex ratios, voting population, school enrollments, cooperative memberships, and so on. However, detailed regional breakdowns, reliable time series, and more refined indices and measures are not issued and may not be available even at the State Statistical Bureau in Peiping. Not all of the published data are accepted uncritically by Peiping, and not all that Peiping accepts will pass critical evaluation on the basis of internal evidence.

* This is a revised version of a paper presented at the annual meeting of the Population Association of America, May, 1958.

The population of Mainland China was 582.6 million as of June 30, 1953, according to the results of the census of 1953.¹ Although many students had regarded the totals reported for earlier censuses and registrations as gross underenumerations, few were prepared for a figure of this magnitude. Peiping at first hailed the results as an assurance of abundant human resources for "socialist reconstruction," but it was soon apparent that the size and rate of growth of the population were more of a liability than an asset in social and economic planning. The government later adopted a policy supporting birth control on grounds which are Malthusian in all but name; it has revised its policy on the expansion of urban centers. Both of these decisions were influenced by population data. It is not plausible to argue that these data were fabricated purely for propaganda purposes. However inadequate, they are probably the best Peiping has. Nevertheless, the limitations of demographic data on Mainland China must be explored before they can be used properly. The main problem in dealing with these materials concerns the defects inherent in the methods of collection and tabulation of the data.

THE CENSUS OF 1953

Most current information on the population of Mainland China comes either from the census of 1953 or from the several attempts at population registration dating from 1951 to the present. The 1953 census is the best source on the size of the mainland population. A standard schedule was used and detailed instructions were issued for the determination of age, the identification of permanent and temporary residents, and the enumeration of persons in special institutional categories. The information sought was elementary: name, age, sex, ethnic group, and relationship to head of household.² The U.S.S.R. census of 1939 was used as a model, and a Soviet census expert was on hand for

consultation during all phases of the work.³

Official confidence in the census results was unbounded. Peiping claimed that a sample check had shown omissions amounting to only 0.255 per cent and duplications amounting to only 0.139 per cent. There is no indication as to how the check was conducted or whether it was actually based on independent field work.

Within China, social scientists expressed cautious scepticism regarding this claim. They noted that the period of preparation for the census was too brief, the staff was not adequately trained, the census depended mainly on reports by heads of household at registration centers instead of on house-to-house canvassing, and tabulation at different administrative levels made it impossible for statisticians at the national level to check the accuracy of the returns.⁴ They further criticized the census authorities for excessive reliance on Russian statistical techniques, for exclusion of the older generation of Western-trained statistical experts, and for imposing security rules which prevented nongovernment scholars from working directly with the data.⁵

Some of the circumstances of the census-taking point to the possibility of an overcount; others strongly support the supposition of an undercount. If heads of household suspected that the census was to be used to determine the size of food and cloth rations or if they expected tax allowances for large numbers of dependents, they would probably have been tempted to inflate their reports wherever they could do so without danger of detection. They may have reported temporary residents as permanent members of their households, or they may have reported as temporarily absent family members

³ Chung Lin and Hsiao Lu, "More on the 1953 Census," *T'ung chi kung tso t'ung hsün*, 10, October 17, 1955, in American Consulate General, Hong Kong, *Extracts from China Mainland Magazines*, 29, March 26, 1956, p. 28.

⁴ Ch'en Ta, *New China's Population Census of 1953 and Its Relations to National Reconstruction and Demographic Research*, Stockholm: The International Statistical Institute, August, 1957, pp. 8-9.

⁵ "Director of the State Statistical Bureau, Hsüeh Mu-ch'iao, Invites University Professors of Economics and Statistics in Peiping and Tientsin to a Symposium to Help in the Rectification of the State Statistical Bureau," *T'ung chi kung tso*, 12 (June, 1957), p. 2.

¹ New China News Agency, Peiping, November 1, 1954, in American Consulate General, Hong Kong, *Current Background*, 301, November 1, 1954, 2 pp.

² "Council Directive on the National Census," New China News Agency, Peiping, April 6, 1953; S. K. Krotevich, "Vsekitaiskaia Perepis' Naseleniia 1953 g." *Vestnik Statistiki*, 5, 1955, pp. 40, 46.

who should have been classed as permanent absentees and so listed on that side of the census schedule which was not to be tabulated. Deceased family members may have been reported as still living. Migrants eager to comply with local authorities may have given data both at the points of origin and of destination rather than risk appearing uncooperative to local census officials. The reporting of data in some areas as of the time of enumeration rather than the census critical date would also have inflated the count, assuming that the population had increased in the interim.

On the other hand, five important aspects of the census-taking procedure suggest that underenumeration was probably more frequent than overenumeration: (1) The census was designed for use in national planning and in the registration of voters for the national elections. Voting registration, however, included an "investigation" to determine political eligibility to vote. Although only a small percentage of voters were disqualified for political reasons, the prospect of investigation must have caused some persons to avoid enumeration.⁶ (2) No measures were taken specifically to ensure the complete enumeration of infants and of females, both usually underrepresented in Chinese population counts. (3) The census-taking lasted one year, from May, 1953, to April, 1954. A few households were enumerated before June 30, the critical date of the census, but the great majority were counted long after the census date. In the case of the early enumerations, the census-takers were directed to verify the count after the census date, making corrections for births and deaths which occurred between the time of enumeration and the census date. Granted that births exceeded deaths, the failure to make a thorough check of these cases would yield a net undercount. In the case of households enumerated after June 30, the census-takers would have less difficulty in excluding infants born after the census date than in gaining information about persons alive on

the census date who had died by the time of the enumeration. (4) In order to make a strictly *de jure* census, enumerators were instructed to exclude temporary residents from the enumeration with the understanding that they would be included in the registration at the place of permanent residence. In all likelihood, a number of people were thus omitted at both places. This would affect both individual migrant workers and whole families. (5) In the case of persons moving during the census period, it is likely that omissions exceeded duplications because of the prolonged duration of the enumeration. Persons who at the time of local enumerations were living in a place other than that in which they were residing on the census date could not be enumerated locally; yet the chances of their being recalled and reported at their former place of residence would have decreased considerably with the lapse of time.

Complete age and sex distributions from the census are not included in the official releases, but come to us indirectly in papers by two leading Chinese demographers read before statistical bodies outside China. In December, 1956, Tai Shih-kuang presented a paper in Calcutta in which he gave a set of sex ratios for irregular age groups (see Table 1).⁷ In a paper sent to the International Statistical Institute in Stockholm in August, 1957, Ch'en Ta provides the only complete percentage age distribution so far available (see Table 2).⁸ Neither of these sources combines age and sex data in a single distribution and neither uses absolute numbers. Both Ch'en's age distribution and Tai's sex ratios show tendencies which have been noted in earlier studies of Chinese populations: a deficit at ages 5-14, an excess of persons at age 45 and over, a sharp rise in sex ratios by ages 10-14, and a marked excess of females after age 55. Yet there are no extreme variations in sex ratio or size between successive age groups, such as are found in the censuses of most Asian countries and in many earlier Chinese population studies. The relative regularity of the 1953 data may mean that in addition to the smoothing effect of the age grouping chosen,

⁶ Teng Hsiao-p'ing, "Census and General Election Completed in China: Population of China Over 600 Million," New China News Agency, Peiping, June 19, 1954, in American Consulate General, Hong Kong, *Survey of China Mainland Press*, 832, June 19-21, 1954, p. 2.

⁷ Tai Shih-kuang, *1953 Population Census of China*, Calcutta: Indian Statistical Institute, December 20, 1956, p. 21.

⁸ Ch'en Ta, *op. cit.*, Table 4, p. 23.

TABLE 1. SEX RATIO BY AGE, MAINLAND CHINA, 1953

Age Groups	Males per 100 Females
All ages.....	107.7
0 years.....	104.9
1 to 2 years.....	106.2
3 to 6 years.....	110.0
7 to 13 years.....	115.8
14 to 17 years.....	113.7
18 to 35 years.....	111.5
36 to 55 years.....	106.8
56 years and over.....	86.7

Source: Tai Shih-kuang, *1953 Population Census of China*, Calcutta: Indian Statistical Institute, December 20, 1956, p. 21.

some further adjustments were made in the data prior to their publication by Ch'en and Tai.

The census also yielded urban and rural population totals. The announcement of 13.26 per cent urban population according to the census differs sharply from the estimates of 20 per cent urban which were current in the 1930s and 1940s. The definition of urban areas released in 1955⁹ and further modified in 1957¹⁰ is much more inclusive than those in use in most other Asian

⁹ "State Council Resolution on the Criteria for Demarcation of Rural and Urban Areas," *T'ung chi kung tso t'ung hsün*, 12, 1955. Cf. *Hsin hua pao yüeh k'an*, 3, 1956, p. 7.

¹⁰ *Handbook on People's China*, Peiping: 1957, pp. 75-80.

TABLE 2. AGE COMPOSITION, MAINLAND CHINA, 1953

Age Groups	Per Cent of Total Population
All ages.....	100.0
0 years.....	3.3
1 to 4 years.....	12.3
5 to 14 years.....	20.3
15 to 24 years.....	17.3
25 to 34 years.....	14.6
35 to 44 years.....	12.0
45 to 54 years.....	9.3
55 to 64 years.....	6.5
65 to 74 years.....	3.4
75 years and over.....	1.0

Source: Ch'en Ta, *New China's Population Census and Its Relations to National Reconstruction and Demographic Research*, Stockholm: International Statistical Institute, August 8-15, 1957, p. 23.

countries. This definition must have been similar to that in force when the census results were tabulated, for subsequent figures show no sudden change in the urban proportion of the population such as might have resulted from a change of definition. The urban figure in the census was slightly inflated by the inclusion of agricultural communities nearby cities, especially those in the vicinity of the larger municipalities. The urban data in Table 3 exclude agricultural communities; this reflects a change in reporting practice since the census. Despite the broad definition and the inclusion of some rural elements, the urban ratio for the

TABLE 3. RURAL-URBAN DISTRIBUTION OF POPULATION, MAINLAND CHINA, 1949-1956 (In Thousands)

End of Year	Total Population		Urban Population			Rural Population		
	Number	Per Cent Increase Over Preceding Year	Number	Per Cent	Per Cent Increase Over Preceding Year	Number	Per Cent	Per Cent Increase Over Preceding Year
1949.....	541,670	...	57,650	10.6	...	484,020	89.4	...
1950.....	551,960	1.9	61,690	11.1	7.0	490,270	88.9	1.3
1951.....	563,000	2.0	66,320	11.8	7.5	496,680	88.2	1.3
1952.....	574,820	2.1	71,630	12.5	8.0	503,190	87.5	1.3
1953.....	587,960	2.3	77,670	13.2	8.4	510,290	86.8	1.4
1954.....	601,720	2.3	81,550	13.6	5.0	520,170	86.5	1.9
1955.....	614,650	2.1	82,850	13.5	1.6	531,800	86.5	2.2
1956.....	627,800	2.1	89,150	14.2	7.6	538,650	85.8	1.3

Source: *T'ung chi kung tso*, June 14, 1957, 11, pp. 24-25. Combined from Tables 2, 3, and 4. *Extracts from China Mainland Magazines*, 91, July 22, 1957, pp. 23-24.

mainland remains one of the lowest in the world.

THE REGISTRATION SYSTEM

The system of population registration now in force on the mainland was first set up in the major cities under regulations promulgated by the Ministry of Public Security in July, 1951. In 1954-1955 the system was extended to rural areas under the central direction of the Ministry of Internal Affairs. The 1955 directive left decisions on details of procedure to the local authorities. Births, deaths, in- and out-migration were to be recorded, but only the year-end population totals were to be reported to higher levels. The reporting was belated and unstandardized. There was no provision for the central collection of vital statistics or migration data.¹¹

The effectiveness of the registration system has diminished because it has been used not only as a means of recording but also of controlling population movements, particularly the rural-to-urban drift. The use of removal permits for migrants seems to have broken down during the urban influx of 1955-1957. Records were falsified or ignored in many cases. Responsible authorities allowed the system to lapse.¹² It was reorganized under a new set of regulations issued in January, 1958, with the Ministry of Public Security in full charge. The police function is now clearly the major emphasis.¹³

Of the registration data thus far published, the most complete series shows total population, rural and urban population, and rates

of growth for the two sectors, 1949-1956.¹⁴ Only the figures for 1954 and 1955 appear to be based mainly on registration reports; the others are estimates or projections. It is therefore difficult to say to what extent the peculiarities of the series stem from defective registration. Some of the fluctuations in urban growth rates may be ascribed to the expulsion from the cities of recent rural migrants and the so-called "voluntary" movement of urban residents to rural areas. However, the decline in the rate of growth of the total population shown for 1955 is contrary to expectations and to official statements about increasing growth rates during this period, and may indicate that increasing numbers of persons were evading registration.

Vital rates have been announced for eight major cities. The more recent figures give crude death rates as low as 6.0 to 8.0, while crude birth rates range from the low thirties to the low forties. Only for the municipality of Peiping are there figures for several successive years. The Peiping figures for total births increase by 50 per cent in 1954 and maintain the new level thereafter; those for total deaths vary from year to year, with a marked low in 1953, the year of the census, and a relative rise in 1954. These anomalies suggest that the system is unstable in coverage. If registration is incomplete in the capital city, it is probably worse elsewhere.

Even as an indication of the level of urban vital rates the data may be misleading. Mortality rates may be seriously affected by the custom of returning to one's native village to die. Similarly, urban birth rates may be inflated by the inclusion in the data of births to mothers from surrounding rural areas at the new city maternity centers. There is no indication that the registration system is prepared to cope with these problems.

SAMPLE SURVEYS

In addition to census and registration data, results of a number of sample surveys have been published. The accuracy check on the census was reportedly based on a 9.2 per

¹¹ "State Council's Directive Concerning Establishment of Permanent System for Registration of Persons," New China News Agency, Peiping, July 2, 1955, in American Consulate General, Hong Kong: *Survey of China Mainland Press*, 1082, July 5-6, 1955, pp. 10-13.

¹² "Stop Blind Rural Exodus," *Jen min jih pao*, Peiping, December 19, 1957; "Peasants in Cities Should Be Urged to Return to Production in Village," *Jen min jih pao*, Peiping, December 16, 1957, in American Consulate General, Hong Kong: *Survey of China Mainland Press*, 1682, January 2, 1958, pp. 2-8.

¹³ "Regulations of the People's Republic of China Governing Household Registration," New China News Agency, Peiping, January 9, 1958, in American Consulate General, Hong Kong: *Survey of China Mainland Press*, 1695, January 21, 1958, pp. 1-12.

¹⁴ "Data on China's Population from 1949 to 1956," *T'ung chi kung tso*, 11, June, 1957, in American Consulate General, Hong Kong: *Extracts from China Mainland Magazines*, 91, July 22, 1957, pp. 22-25.

TABLE 4. COMPOSITION AND RESULTS OF SAMPLE CHECK ON ACCURACY OF 1953 CENSUS OF MAINLAND CHINA

Size of sample.....	52,953,400
Per cent of total population	9.2
Composition of sample..	343 counties and cities in 23 provinces, plus 5 other cities and 1 autonomous area.
Omissions.....	2.55 per thousand
Duplications.....	1.39 per thousand
Net underenumeration...	1.16 per thousand

Source: Ch'en, *op. cit.*, p. 8.

cent sample of the population directly enumerated. The sample included parts of 343 counties and cities in 23 provinces, plus five other cities and one ethnic minority area. The results were tabulated separately for the rural and urban components, and the latter was in turn subdivided into institutional and noninstitutional population.¹⁵ There are no indications that any attempt was made to secure a random sample; administrative convenience was apparently the basis of selection (see Table 4).

The often cited birth rate of 37, death rate of 17, and natural increase rate of 20 per thousand per year were based on another survey. In this case the sample comprised 29 large and medium-sized cities, the entire province of Ninghsia, ten counties from other provinces, and parts of 35 other counties.¹⁶ The total population in the sample areas was 30,180,000. About 70 per cent of the counties in China range in size from 200,000 to 300,000, although some run as high as 1,000,000.¹⁷ Ninghsia, which had a population of just over 900,000 in 1953,¹⁸ was well above the national average in the proportion rural. Yet the urban component of this sam-

ple is more than half of the total, whereas the urban population of China is about one-seventh of the total population. Once again it appears that administrative convenience rather than scientific design determined the composition of the sample (see Table 5).

The survey of 30 million which yielded the 2.0 per cent natural increase rate incorporated two earlier surveys of vital rates. The first covered two small rural areas, which were studied during the period from June, 1951, to June, 1953.¹⁹ This survey found a birth rate of 33.64, a death rate of 19.34, and a natural increase rate of 14.30. The second sample included these two areas together with five rural counties and part of a sixth county, plus the 29 municipalities referred to above, making a predominantly urban sample.²⁰ This sample shows a birth rate of 38, a death rate of 11, and a natural increase rate of 27. We see here the cumulative formation in three stages of the sample of 30,180,000. The changes in vital rates from one stage to the next suggest that the addition of rural components consistently raises the death rate and lowers the birth rate. It may be inferred that no attempt has been made to weight the sample in order to achieve the rural-urban ratio of the country as a whole. It is important to bear in mind that this is the sample upon which the widely quoted increase rate of 2.0 per cent per year is based.

Not only are the government surveys of vital rates defective in sample design, but there is good reason also to suspect underenumeration of births and deaths. Ch'en Ta has published the results of 16 surveys of "fairly high accuracy," which he selected from a still larger number conducted during 1951-1954.²¹ He does not reveal his sources, but they are presumably the surveys upon which the government announcements were based. The population involved in the 16 surveys totalled 1,800,000 and was predominantly rural. The birth rate for this popula-

¹⁵ Ch'en Ta, *op. cit.*, p. 8.

¹⁶ Pai Chien-hua, "600 Million People—A Great Strength for Socialist Construction of China," *Jen min jih pao*, Peiping, November 1, 1954, in American Consulate General, Hong Kong: *Survey of China Mainland Press*, 926, November 11-12, 1954, p. 33; Ma Yin-ch'u, "A New Theory of Population," *Jen min jih pao*, Peiping, July 5, 1957, in American Consulate General, Hong Kong: *Current Background*, 469, p. 1.

¹⁷ *Handbook on People's China*, Peiping: 1957, p. 78. See also S. B. Thomas, *Government and Administration of Communist China*, New York: Institute of Pacific Relations, 1955, pp. 86-87.

¹⁸ Ch'en Ta, *op. cit.*, p. 10.

¹⁹ "Census and Registration Work Completed in Areas with 60 Percent of Population in China," New China News Agency, Peiping, March 10, 1954, in American Consulate General, Hong Kong: *Survey of China Mainland Press*, 766, March 13-15, 1954, p. 27.

²⁰ *Ibid.*

²¹ Ch'en Ta, *op. cit.*, p. 14 and Table 6, p. 25.

TABLE 5. COMPOSITION AND RESULTS OF SAMPLE SURVEYS ON VITAL RATES, MAINLAND CHINA, 1951-1954

Sample	Size of Sample (thousands)	Composition	Period of Survey	Birth Rate	Death Rate	Natural Increase Rate
1*	30,180	29 medium and large cities; province of Ninghsia; 10 counties; parts of 35 other counties (1 district, 2 towns, 58 townships, 7 villages)	(1951-1954?)	37	17	20
2**	Not given	29 cities, 5 counties, 2 townships, 1 village	1951-1953	38	11	27
3**	Not given	2 townships	1951-1953	33.64	19.34	14.3
4†	1,810	7 counties, 18 townships in 7 provinces	1951-1954	41.6	21.0	20.6

*Source: Pai Chien-hua, "600 Million People—A Great Strength for Socialist Construction in China," *Jen min jih pao*; Peiping, November 1, 1954, in American Consulate General, Hong Kong: *Survey of China Mainland Press*, 926, November 11-12, 1954, p. 33.

**Source: "Census and Registration Work Completed in Areas with 60 Per Cent of Population in China," New China News Agency, Peiping, March 10, 1954, in American Consulate General, Hong Kong: *Survey of China Mainland Press*, 766, March 13-15, 1954, p. 27.

†Source: Ch'en Ta, *op. cit.*, Table 6, p. 25. In four component surveys, covering two counties and three townships, there is evidence of undercounting of births and deaths. Excluding these, the remaining population of 1,227,498 had a birth rate of 46.7, a death rate of 22.2, and a natural increase rate of 24.5 per thousand.

tion was 41.6, the death rate 21.0, and the natural increase rate 20.6. Four of the surveys, covering one-third of the population of the sample, revealed birth and death rates so far below the average as to suggest that information on the number of births and deaths was incomplete. Ch'en observes that birth rates may be low in less prosperous agricultural areas, but in such cases the death rates should be somewhat higher than average. Since the data on these four counties also show lower death rates, underenumeration is strongly indicated. Setting these four surveys aside, the remaining two-thirds of the population had a birth rate of 46.7, a death rate of 22.2, and a natural increase rate of 24.5.

The evidence concerning sampling method and completeness of registration in the surveys of vital rates points to higher country-wide birth and death rates than those which have been endorsed by Peiping. On the rate of natural increase the evidence is ambiguous. A rate lower than 2.0 per cent is implied by the 30 million sample; this is contradicted by Ch'en's data. At present there seems to be no way of resolving this dilemma.

For some time after 1953 the 2.0 per cent

natural increase rate was officially accepted in Peiping. In 1957, however, an official spokesman reported a rate of 2.2 per cent.²²

No natural increase rates have been announced for rural China. The urban rate has been estimated by Peiping to be 3.0 per cent, well above the national average. Higher rates have been reported for individual cities. The urban population has gained heavily through the migration of people in the child-bearing ages, thus raising the urban birth rate. Control of disease, particularly as it affects infant and maternal mortality, is greater in the cities, thus lowering the death rate.

CONCLUSIONS

Clearly, Peiping makes use of its demographic information in the formulation of policy and planning. Despite strong ideological indisposition, a radical shift was made in the official policy on birth control in response to what was discovered about popula-

²² "Exercise Appropriate Birth Control," *Jen min jih pao*, Peiping, March 5, 1957, in American Consulate General, Hong Kong: *Survey of China Mainland Press*, 1487, March 12, 1957, p. 7.

tion size and rates of growth through the census, registration, and vital rates surveys. Again, registration data and certain economic statistics disclosed that the urban labor force was increasing more rapidly than the capacity of urban industry to absorb it. Registration statistics also showed an increasing urban nonproductive population. Consequently, current policy discourages migration of rural residents to the cities, and large numbers of urban residents are being drafted for work in the countryside.²³ These major

policy decisions testify to the acceptance of official population data.

The current demographic statistics of Mainland China have many defects. Nevertheless, as with other defective data, they need not be rejected altogether. With proper allowance for known or perceptible defects, the data from Communist China yield much useful information.

²³ Szu Keng-sheng, "Several Questions for Discussion and Study on the Statistics of China's Population," *Tung chi kung tso*, 5, March, 1957, in American Consulate General, Hong Kong: *Extracts from China Mainland Magazines*, 78, April 15, 1957, p. 18; Wang Kuang-wei, "How to Organize Agri-

cultural Labor Power," *Chi hua ching chi*, 8, August 9, 1957, in American Consulate General, Hong Kong: *Extracts from China Mainland Magazines*, 100, September 23, 1957, pp. 11-15; Chang Ching-wu, "Why Must We Reduce Urban Population," *Daily Worker*, Peiping, January 4, 1958, in American Consulate General, Hong Kong: *Survey of China Mainland Press*, 1693, January 17, 1958, pp. 7-9. Cf. *Union Research Service*, 9, 15, November 19, 1957; 10, 1, January 3, 1958; 10, 3, January 10, 1958.

AN INDEX OF SOCIO-ECONOMIC RANK OF CENSUS TRACTS IN URBAN AREAS *

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An index of socio-economic rank of census tracts, utilizing U. S. Census data on occupation, education, income, and non-white population, is presented. The index, which requires minimal computation, distinguishes between tracts falling into a General Pattern of socio-economic rank and Deviant Patterns. Comparison with the Shevky-Bell social rank scale for San Francisco-Oakland data shows for both General Pattern and Deviant tracts, a high correlation with Shevky-Bell scores. While General Pattern tracts have low urbanization scores on the Shevky-Bell index, Deviant tracts generally have high scores. For Chicago data, at each socio-economic level, General Pattern tracts show a lower residential turnover than Deviant tracts. Trade cities have a higher proportion of Deviant tracts than manufacturing cities.

IN the face of sustained urban growth, both in population and geographical space, there is a continuing need for standards by which cities and their components can be classified meaningfully. The problem of classification leads into measurement and the construction of indices. This

paper is concerned with describing and testing an index of socio-economic rank of census tracts.

A major deficiency of present indices of socio-economic rank is that they are not regarded primarily as a kind of homogeneity index.¹ As homogeneity indices, they should

* The index described in this paper was developed for the study reported in Bernard Farber, "Effects of a Severely Mentally Retarded Child on Family Integration," *Monographs of the Society for Research in Child Development*, 24, 2 (1959). This study was supported by a grant from the Mental Health Fund of the Illinois Department of Public Welfare to the Institute for Research on Exceptional Children, University of Illinois. We wish to express our gratitude to Joseph Gusfield, University of Illinois, for comments and suggestions.

¹ See, e.g., Wendell Bell, "Economic, Family, and Ethnic Status: An Empirical Test," *American Sociological Review*, 20 (February, 1955), pp. 45-52; Maurice D. Van Arsdol, Jr., Santo F. Camilleri, and Calvin F. Schmid, "The Generality of Urban Social Area Indexes," *American Sociological Review*, 23 (June, 1958), pp. 277-284; index developed by Albert Mayer in Arnold M. Rose, "Distance of Migration and Socio-Economic Status of Migrants," *American Sociological Review*, 23 (August, 1958), pp. 420-423. Cf. Otis Dudley Duncan and Beverly

take into account the degree of similarity of the residents of the area on specific characteristics. Insofar as existing indices are based on such an item as median rent, proportion of professionals in the labor force, or proportion of unskilled workers, the characteristic often pertains to only a minority of the adult residents in the particular area. There is no explicit description in the gross socio-economic score of the extent of homogeneity among residents.

A second deficiency of indices of socio-economic rank is that, with the omission of a statement about homogeneity, the comparison between scores assigned to particular areas provides relatively little information about the areas. Therefore, the scores do little to clarify the nature of the dimension being measured. For example, if 15 per cent of the adults in a particular census tract and 20 per cent in a second tract have less than an eighth-grade education, these figures merely tell us that one tract has relatively more adults who stopped their schooling at the eighth grade. Conceivably, in every tract in the city, adults with an eighth-grade education may constitute a minority. Furthermore, when this raw percentage figure is then used in the computation of a gross socio-economic score, the total score does not describe the area adequately enough to indicate the specific ways in which one area differs from another. Nothing is revealed in the total socio-economic score itself about the education of the *majority* of adult residents in one tract as distinguished from the *majority* in another tract.

A third deficiency of indices of socio-economic rank is that since no clear statement of the dimension being studied is made, a system must be devised for weighting various attributes on a general factor called socio-economic rank or its equivalent. The weighting is ordinarily performed through factor analysis, a procedure having several consequences: (a) The complexity of computation in finding and then applying weights is considerable. (b) The number of categories used in the distribution of socio-economic levels is a matter of the investigator's discretion. (c) The use of numerical total scores

based on average weights rather than a score taking into account patterning of attributes in the area makes no provision for sub-classes of socio-economic rank.² Hence, if loadings on a second factor are used to provide weights for a second dimension, according to the principle of orthogonality, the second dimension is *co-ordinate* with the first rather than a *subordinate* classification scheme.

An attempt was made to construct an index of socio-economic rank which would avoid the usual deficiencies of such indices. The specific characteristics of this index, however, reflect the original purpose of the index: to estimate characteristics of non-respondent families from their addresses on mailing lists. The index described in this paper is based essentially on three principles:

1. Dimensionality is regarded from a Guttman rather than factor analysis perspective.³ That is, the aim was to develop a scale which assumed an underlying continuum that could be identified. With this aim, a problem arose concerning the interpretation of patterns which did not conform to the unidimensional scale. Should these deviant patterns be regarded as meaningful in their own right or should they be considered as random deviations? An answer to this problem was suggested by the results of applications of the index discussed below.

2. In some instances, as in the study of sampling bias, one problem is that of predicting attributes of individuals on the basis of characteristics of their area of residence. If nothing is known of a given individual except his area of residence, the best prediction for the individual is an attribute characteristic of most people in that area.⁴ If medians are used as a basis, an individual can be classified as to whether the area from which he is drawn falls above or below the median for the city on a given attribute. For an attribute (for example, median income) of the census tract as compared with the median (income) for the city, there would

² Cf. Amos Hawley and Otis D. Duncan, "Social Area Analysis: A Critical Appraisal," *Land Economics*, 33 (November, 1957), pp. 337-345.

³ Samuel A. Stouffer *et al.*, *Measurement and Prediction*, Princeton: Princeton University Press, 1950.

⁴ Paul Horst *et al.*, *The Prediction of Personal Adjustment*, Bulletin 48, New York: Social Science Research Council, 1941.

Duncan, "Residential Distribution and Occupational Stratification," *American Journal of Sociology*, 60 (March, 1955), pp. 493-503.

be a probability of greater than 50 per cent that any resident picked at random in that area has the predicted classification on that attribute (high or low income). Hence, the index is based on the principle of maximum probability.

3. Frequently, census tracts with a large non-white population are omitted from the computations of socio-economic status or a supplementary index on segregation is introduced in the description of socio-economic rank. In either case, present indices are not parsimonious in describing the ecological patterning of the community. The non-white population is an organic part of the city and therefore the index should treat non-whites as part of the general status system. Hence, within the restrictions of unidimensionality and the principle of maximum probability, an attempt was made to include an item on non-white residents in the index.

The remaining sections of this paper are devoted to a description of the items used in the index, a discussion of the scoring procedure, an examination of the ordering of scale items, and an investigation into the meaning of a general pattern *versus* deviant patterns dichotomy. Finally, to indicate a potential application of the index, the numerical distributions of general pattern tracts and deviant tracts of manufacturing and trade cities are compared.

In the course of examining the index, comparisons were made with the Shevky-Bell indices of social rank and urbanization.⁵ These were used as a bench mark for evaluating the index developed at the University of Illinois because of their wide application. Here, the index constructed at Illinois is referred to as the Farber-Osoinach index.

SELECTION OF ITEMS IN INDEX

Initially, the Farber-Osoinach index was developed to estimate characteristics of non-respondent families in a research project. Therefore, only those items published in Census data for 1950 on which all adults in families can be classified were used in the index.⁶ All adults in families are included in

classifications referring to (1) whether or not the breadwinner is a white-collar worker,⁷ (2) the number of school years completed by adults, (3) median income, and (4) race.

The initial intention of the Farber-Osoinach index was to focus upon family life; thus the items chosen for the index were those which generally discriminate between the various styles of living and life chances found in different areas of the city.⁸ Since in the city, persons ordinarily live in nuclear families, the residence of the family unit seems appropriate for describing socio-economic variables associated with the characteristic life-style of most persons.⁹

Under ideal conditions, data would be gathered for all four items on the same basis. If we regard the family as the important unit in style of living, then all information should pertain to families; for example, median *family* income, median education of *married* persons, occupation of head of *family*, and race of *families*. The classification of families by race (or education) would involve the classification of married persons and widowed or divorced parents by race (or education).

Specifically, median family income, rather than individual income, is used in the index for the following reasons: In those tracts where many husbands and wives may both be working at low status jobs, the median family income may be high; at the same time, the proportion of persons in white-collar jobs may be low. This may be contrasted with other tracts of comparable family income in which a white-collar worker is generally the only breadwinner. In some respects, the styles of life prevalent in the

⁷ A white-collar occupation was defined as one which, according to the Census system of coding major occupational groups, has a first digit of 0, 2, 3, or 4 in their three-digit code. Occupations with a first digit of 5, 6, 7, or 9 were regarded as non-white-collar. Bureau of the Census, *Alphabetical Index of Occupations and Industries: 1950*, Washington, D. C.: U. S. Government Printing Office, 1950, p. vi.

⁸ See Harold Pfautz, "Current Literature on Social Stratification: Critique and Bibliography," *American Journal of Sociology*, 58 (January, 1953), pp. 391-418. Cf. James A. Quinn, *Human Ecology*, New York: Prentice-Hall, 1950, pp. 138-162.

⁹ On the family as a unit for ecological study, see Amos H. Hawley, *Human Ecology*, New York: Ronald, 1950, pp. 206-233.

⁵ Eshref Shevky and Wendell Bell, *Social Area Analysis*, Stanford: Stanford University Press, 1955.

⁶ Bureau of the Census, *1950 Census of Population, Vol. III, Census Tract Statistics*, Washington: U. S. Government Printing Office, 1952.

two areas would be similar because of income; in other respects, because of occupation, the life-styles would be different. In addition, both of these areas would differ from the area in which there tends to be only one breadwinner in the family holding a low status job. If individual income were used as a criterion for classification, these distinctions could not be made.

As not all data were available on a family basis, those classifications closest to the ideal were used in the construction of the present index.

On the basis of 1950 Census data, the following criteria were used in applying the index to census tracts in cities: (1) Whether the family income in 1949 for the census tract fell above or below the median for the city; (2) whether the median years of schooling for adults 25 years of age or over was greater or less than the median for the city in 1950; (3) whether or not the majority of those in the labor force worked in white-collar jobs; and (4) whether the majority of the inhabitants in the tract were white or non-white.

If the general dimension of socio-economic rank reflects patterning of *family* life-styles, however, predictions concerning inhabitants of areas with a high proportion of unattached individuals (that is, not living in families) may be faulty. In such areas the proportion of unrelated individuals may be large enough to affect especially the classification on occupation, education, and race in the index. Areas with a high proportion of unattached individuals generally are characterized by considerable residential mobility and score high in urbanization (or low in family status) on the Shevky-Bell index. The efficacy of the Farber-Osoinach index in identifying these areas is indicated below.

SCORING SYSTEM

The following arbitrary scoring system was used for classifying areas on each item: when the median family income for an area fell above the median for the city, the area received the score of *two* on this item; when the median income for an area was below the city median, a *zero* was assigned. The same system was used for scoring an area on median years of education of adults of

25 or over. An area in which the majority of those in the labor force was classified as white-collar received a *two*; one in which white-collar workers were a minority, a *zero*. Similarly, an area in which the majority of inhabitants were white was scored *two* and where the minority were white the area received a *zero*.

If the majority category is used as the basis for classification of individuals, the problem arises of what to do when no majority category exists. Classification for two items in the scale (years of school completed and income) is based on census tract median, not the city median, and the other two items (white-collar workers and non-whites) are based on majorities in the area. When the area falls on the median for the city in years of school completed or income, no prediction can be made. Similarly, when half the population in the area is non-white or half the labor force is white-collar, there is no basis for prediction. In such cases, a middle score of *one* is assigned, which indicates in effect that no prediction is made for the item. The middle score when no majority exists gives rise to the scoring system used for Chicago in 1950, presented in Table 1.

There is evidence that occupation, rather than income, should be regarded as the anchor of socio-economic status. For example, in the Warner ISC scale, occupation is given

TABLE 1. WEIGHTS GIVEN TO ITEMS ON FARBER-OSOINACH INDEX APPLIED TO CHICAGO 1950 CENSUS TRACT DATA

Item		Weight
a. Percentage of white-collar workers in labor force:		
	66.8% or over	4
	66.5-66.7	3
	50.5-66.4	2
	49.5-50.4	1
	49.4 or under	0
b. Median years of school completed, persons 25 years and older:		
	9.6 or over	2
	9.5	1
	9.4 or under	0
c. Median income of families:		
	\$4056 or over	2
	3856-4055	1
	3855 or under	0
d. Percentage of residents who are non-white:		
	50.5% or over	0
	49.5-50.4	1
	49.4 or under	2

a greater weight than income.¹⁰ Similarly, Kahl and Davis found that occupation, whether classified according to the Warner scheme or the Census categories, has higher loading than income in their factor analysis of socio-economic indices.¹¹ Sociologists who view status in group rather than aggregate terms seem to find occupational setting efficient in making predictions about life-style in families.¹² And Duncan and Duncan found that in spite of lower income, clerical workers were concentrated less in low-rent areas than were craftsmen and foremen.¹³ Thus, although occupational status and income are highly correlated, the evidence suggests that occupation be regarded as the primary criterion of socio-economic rank.

In order to increase the ability of the index to discriminate between social ranks, an attempt was made to take advantage of the upper extremes in the range of proportion of white-collar workers in the labor force. It was believed, however, that the grossness of the data precluded very fine gradations. Hence, only a single additional category was sought to identify the census tracts in the extremes of socio-economic rank.

In order to identify tracts in which the proportion of persons in white-collar occupations is very high, an additional cutting point was introduced. As shown in Table 1, the scale was able to discriminate between those tracts in which white-collar workers constitute a simple majority of the labor force from those in which more than two-thirds of the workers are white-collar. In these latter extreme tracts, a prediction of a family head as a white-collar worker could be made with greater confidence than in others, without violating the unidimensionality of the scale. Thus the prediction of white-collar workers is made with less risk in these areas and the scoring is modified to take account of the greater confidence in the prediction.

Because of the specific application of the

Farber-Osoinach index to a study of sampling bias, it was not considered important to distinguish between tracts in which non-whites are a simple majority and those in which almost all residents are non-whites. In future application of the index, perhaps, a special category for predominantly non-white tracts should be employed.

ORDERING OF THE SCALE ITEMS

The ordering of the scale items is reflected in intercorrelations found between them in various studies. According to one investigation, there are relatively high correlations between the proportion of the population classified as professionals, proprietors, managers, and officials and median income in census tracts. Even higher correlations were found between the proportion of these occupations and median grade of school completed. On the other hand, negative correlations were found between income, education, and occupation and proportion of non-whites.¹⁴ It seems plausible, therefore, to posit an underlying continuum of census tracts by socio-economic status of the residents within the census tracts.

The scale types for Chicago, 1950, appear in Table 2.¹⁵ Of the 935 census tracts in the city, 668 were scored according to the four items. The population of the remaining 267 tracts was too small to permit classification; data on median income were not available. As Table 2 indicates, not all census tracts fit the pattern which was supposed to reflect the stable socio-economic status system of the community.

COMPARISON BETWEEN FARBER-OSOINACH SCORES AND SHEVSKY-BELL SOCIAL RANKS

The question may be raised as to whether or not the scores on the Farber-Osoinach

¹⁰ W. Lloyd Warner et al., *Social Class in America*, Chicago: Science Research Associates, 1949.

¹¹ Joseph A. Kahl and James A. Davis, "A Comparison of Indexes of Socio-Economic Status," *American Sociological Review*, 20 (June, 1955), pp. 317-325.

¹² See, e.g., Daniel R. Miller and Guy E. Swanson, *The Changing American Parent*, New York: Wiley, 1958.

¹³ Duncan and Duncan, *op. cit.*, p. 499.

¹⁴ Calvin F. Schmid, Earle H. MacCannell, and Maurice Van Arsdol, Jr., "The Ecology of the American City: Further Comparison and Validation of Generalizations," *American Sociological Review*: 23 (August, 1958), pp. 392-401.

¹⁵ Data from Philip M. Hauser and Evelyn M. Kitagawa, editors, *Local Community Fact Book for Chicago, 1950*, Chicago: Chicago Community Inventory, University of Chicago, 1953; and Bureau of the Census, *1950 Census of Population, Chicago, Illinois, Census Tracts* (Vol. III, Bulletin P-D 10), Washington: U. S. Government Printing Office, 1952.

TABLE 2. SOCIO-ECONOMIC RANKING OF CHICAGO COMMUNITY AREAS AND CENSUS TRACTS BY SCALE PATTERNS, 1950

Socio-Economic Ranking	Per Cent White Collar	Median Yrs. Educ.	Median Family Income	Per Cent Non-White	No. of Community Areas	No. of Census Tracts
General Pattern						
10	4	2	2	2	8	75
9	3	2	2	2	0	0
8	2	2	2	2	12	80
7	1	2	2	2	2	9
6	0	2	2	2	6	38
5	0	1	2	2	2	6
4	0	0	2	2	19	123
3	0	0	1	2	3	52
2	0	0	0	2	9	143
1	0	0	0	1	1	2
0	0	0	0	0	7	67
Total Areas Conforming to General Pattern					69	595
Deviant Areas						
9	4	2	1	2	0	2
7	2	2	1	2	0	11
7	2	1	2	2	0	1
6	2	2	0	2	1	11
6	2	0	2	2	0	1
6	1	1	2	2	0	1
6	1	2	1	2	0	1
5	1	2	0	2	0	2
5	0	2	1	2	0	5
5	1	0	2	2	0	2
5	2	0	1	2	0	2
4	0	2	0	2	3	15
4	2	0	0	2	0	1
4	0	1	1	2	0	2
3	0	1	0	2	1	2
2	0	2	0	0	0	14
Total Deviant Areas					5	73
Total Areas Scored					74	668

index of socio-economic rank are comparable to those of other indices. The availability of social rank scores on the Shevsky-Bell index for the San Francisco-Oakland area led to a comparison of these two indices.¹⁶ The results are shown in Tables 3 and 4. Of the 249 tracts in the San Francisco-Oakland area, in seven cases the population is too small to permit computation of scores on the Farber-Osoinach index.

Table 3 shows those census tracts which fall into the General Pattern on the Farber-Osoinach index. The census tracts are classified by both Shevsky-Bell social rank and Farber-Osoinach scores. All 44 cases in the

9-10 categories on the Farber-Osoinach index, which are distinguished from the 7-8 categories only by the proportion of persons classified in white-collar occupations, are in the top Shevsky-Bell social rank. No tract with a score of 6 or less on the Farber-Osoinach scale has a Shevsky-Bell social rank score as high as 4. At the other extreme, no census tract with a Farber-Osoinach score of 5 or over has a low social rank score on the Shevsky-Bell index. Possibly, if those tracts with an extremely large proportion of non-whites were treated separately, the results would be even more definitive at the lower end of the Farber-Osoinach scale. For the 186 tracts falling into the General Pattern on the Farber-Osoinach index, the Pearsonian correlation coefficient between the socio-economic rank on this index and the raw score on social rank in the Shevsky-

¹⁶ Shevsky and Bell, *op. cit.* The results are reported in John C. Osoinach, "An Index of Socio-Economic Variables Affecting Spatial Distribution in American Cities, An Evaluation Study," Ph.D. thesis, University of Illinois, 1958.

TABLE 3. CLASSIFICATION OF CENSUS TRACTS IN SAN FRANCISCO-OAKLAND AREA, 1950, BY SOCIAL RANK SCORES ON THE SHEVKY-BELL AND FARBER-OSOINACH INDICES, FOR CENSUS TRACTS FALLING INTO GENERAL PATTERN OF FARBER-OSOINACH SCORES

Socio-Economic Rank on Farber-Osoinach Index	Social Rank on Shevky-Bell Index			Total No. of Census Tracts
	4	3	1-2*	
9-10	44	—	—	44
7-8	13	31	—	44
5-6	—	8	—	8
3-4	—	24	16	40
1-2	—	14	22	36
0	—	6	8	14
Total	57	83	46	186

* Includes a single case in Category 1 of the Shevky-Bell scale.

Bell index is .88. Thus the ordering of the items in the General Pattern on the Farber-Osoinach index reflects social rank as indicated by the Shevky-Bell scale.

Table 4 indicates the comparability between the two indices for census tracts which deviate from the General Pattern on the Farber-Osoinach scale. The census tracts falling into the 8-9 category of the Farber-Osoinach index are all in the top Shevky-Bell social rank category; and the tracts with a score of 5 or less on the Farber-Osoinach scale are low in social rank on the Shevky-Bell scale. For the census tracts classified as Deviant on the former index, the Pearsonian correlation coefficient between the socio-economic (or social) rankings on the two indices is .76. Hence, whether census tracts fall into the General or Deviant Patterns in the Farber-Osoinach scale, a high score on the Farber-Osoinach index is generally comparable to a high score on the Shevky-Bell index.

GENERAL PATTERN AND DEVIANT TRACTS

Ordinarily, a coefficient of reproducibility is viewed as evidence that an underlying unidimensional scale exists. With only three dichotomous items and one trichotomous item in the Farber-Osoinach scale, it did not seem advisable to compute such a coefficient.¹⁷ Instead, it was assumed that non-scale types do not occur in a random manner, but have a special significance in the spatial distribution of the city.

If it is assumed that the General Pattern of scale types (Table 2) reflects a continuum of socio-economic status, then the deviant types should represent tracts which are different in character. For example, in Chicago 1950, it was found that five of the 74 community areas (Kenwood, Lincoln Park, Near North Side, Woodlawn, and Englewood) were deviant types. Hauser and Kitagawa report that each of these five areas was undergoing a substantial Negro invasion in

¹⁷ Stouffer, *op. cit.*, p. 79.

TABLE 4. CLASSIFICATION OF CENSUS TRACTS IN SAN FRANCISCO-OAKLAND AREA, 1950, BY SOCIAL RANK SCORES ON THE SHEVKY-BELL AND FARBER-OSOINACH INDICES, FOR CENSUS TRACTS DEVIATING FROM THE GENERAL PATTERN OF FARBER-OSOINACH SCORES

Socio-Economic Rank on Farber-Osoinach Index	Social Rank on Shevky-Bell Index		Total No. of Census Tracts
	4	2-3*	
8-9	15	0	15
6-7	10	11	21
1-5	0	20	20
Total	25	31	56

* Includes three cases in Category 2 on the Shevky-Bell Scale.

TABLE 5. PROPORTION OF RESIDENTS, AGED ONE YEAR OR OVER, WHO LIVED IN THE SAME HOUSE IN 1949 AND 1950, IN GENERAL PATTERN AND DEVIANT CENSUS TRACTS, CHICAGO, 1950

Socio-economic Rank of Census Tracts	General Pattern Census Tracts		Deviant Census Tracts	
	No. of Tracts	Proportion of Residents Living in Same House in 1949 as in 1950	No. of Tracts	Proportion of Residents Living in Same House in 1949 as in 1950
10	74	.845
9	2	.701
8	79	.889
7	9	.886	12	.706
6	38	.874	14	.760
5	6	.872	11	.827
4	123	.892	18	.770
3	52	.896	2	.841
2	92	.860	14	.790
1	2	.776
0	67	.848

1950.¹⁸ This finding suggests that the deviant types should be high both in residential mobility and in urbanization score on the Shevky-Bell index.

For the city of Chicago, the proportion of residents, aged one year or over, who lived at the same address in 1950 and 1949, was computed for each census tract.¹⁹ The stability of residence in General Pattern census tracts was then compared with that in Deviant census tracts. The results are shown in Table 5. At each socio-economic rank, the relative amount of turnover in residence in the tracts classified as Deviant is greater than residential turnover in General Pattern tracts.²⁰

¹⁸ Hauser and Kitagawa, *op. cit.*

¹⁹ Bureau of the Census, 1950 Census, Vol. III, Bulletin P-D10, *op. cit.*

²⁰ Cf. Peter H. Rossi, *Why Families Move*, Glencoe, Ill.: Free Press, 1955, pp. 180-181.

A comparison was also made between patterns on the Farber-Osoinach index and Urbanization scores on the Shevky-Bell index for San Francisco, Oakland, and adjacent areas.²¹ The results appear in Table 6. Of the 44 census tracts of very high urbanization (D category) on the Shevky-Bell index, 27 were Deviant tracts on the Farber-Osoinach index. However, only 7 of the 104 tracts in low urbanization categories on the Shevky-Bell index were Deviant. The Chi square for this table is 51.95; with two degrees of freedom, this figure is significant at the .001 level. The San Francisco data thus provide additional support to the contention that the Deviant areas are qualitatively different from the General Pattern.

This analysis of General Pattern and Deviant tracts suggests that the socio-economic

²¹ See Osoinach, *op. cit.*

TABLE 6. CLASSIFICATION OF CENSUS TRACTS IN SAN FRANCISCO AND OAKLAND ON SHEVKY-BELL URBANIZATION SCALE AND ACCORDING TO GENERAL OR DEVIANT PATTERNS ON FARBER-OSOINACH SCALE

Classification on Shevky-Bell Urbanization	Classification of Census Tracts on Farber-Osoinach Scale		Total No. of Tracts
	No. of Tracts in General Pattern	No. of Tracts in Deviant Pattern	
D (High urbanization)	17	27	44
C	72	22	94
A-B* (Low urbanization)	97	7	104
Total	186	56	242

* Includes 9 census tracts in Category A.
Chi square = 51.95; $p < .001$.

TABLE 7. PROPORTION OF CENSUS TRACTS DEVIATING FROM THE GENERAL PATTERN IN FIFTEEN MANUFACTURING AND TRADE CITIES, 1950

Cities	No. of General Tracts in City	No. of Deviant Tracts in City	Proportion of Tracts Which Are Deviant
Manufacturing Cities			
Bridgeport, Conn.	42	7	.143
Chattanooga, Tenn.	34	2	.056
Dayton, Ohio	53	8	.113
Flint, Mich.	43	14	.246
Louisville, Ky.	78	7	.082
Paterson, N. J.	53	5	.086
Toledo, Ohio	49	5	.093
Total	362	48	
Means	51.7	6.9	.117
Trade Cities			
Dallas, Texas	76	19	.200
Denver, Colo.	35	15	.300
Miami, Fla.	63	9	.125
Norfolk, Va.	46	11	.193
Portland, Ore.	43	16	.271
Sacramento, Calif.	58	12	.171
Seattle, Wash.	126	31	.197
Spokane, Wash.	26	8	.235
Total	473	121	
Means	59.1	15.1	.204

nomic rank of General Pattern tracts is based on a relatively stable population. Population estimates for General Pattern tracts therefore can be made with less risk than can similar estimates for Deviant tracts. With more population movement in Deviant tracts, there is a greater probability of heterogeneity in the make-up of the population of the tract. Not only in an ecological sense, but perhaps also in terms of status, the Deviant tracts are interstitial.

CENSUS TRACT PATTERNS AND TYPES OF CITIES

One of the claims made for the Farber-Osoinach index is that, depending upon specific research interests, it can be used to compare cities in a variety of ways. If the distinction between General and Deviant tracts is meaningful, for example, the proportion of tracts classified as General in a given city should reflect a basic activity of the city. To explore this hypothesis, intercity comparisons of General and Deviant patterns were examined according to the Farber-Osoinach index.

Because of obvious differences in economic function, trade and manufacturing cities were compared on the basis of proportion of their

census tracts falling into the General Pattern. The cities chosen were (a) under one million in population in 1950, (b) divided into 25 or more census tracts, and (c) had at least a seven per cent spread between the percentage of persons working in manufacturing and in wholesale and retail trade.²² A manufacturing city was defined as one with a larger percentage of its labor force in manufacturing than in trade in 1950, and a trade city as one with the opposite work-force balance. The cities which met the required criteria are listed in Table 7.

As shown in Table 7, the mean proportion of census tracts in the seven manufacturing cities deviating from the General Pattern of the index is .117; the comparable figure for the eight trade cities is .204. Thus the trade cities show an average of nine per cent more of their census tracts deviating from the General Pattern than do manufacturing cities. These results suggest that the distinction between General Pattern and Deviant tracts is useful in indicating effects of city function upon residential distribution. (Inci-

²² The data were taken from Bureau of the Census, *1950 Census of Population, Vol. II, Characteristics of the Population, Part I, U. S. Summary*, and *Vol. III, Census Tract Statistics*.

dentally, only six per cent of the census tracts in manufacturing cities had populations too small to permit classification on the Farber-Osoinach index; twelve per cent of the tracts in trade cities could not be classified.)

Two interpretations can be made of these results. One is an extrapolation of the findings pertaining to the residential turnover in Deviant tracts in Chicago and Shevsky-Bell urbanization scores for such tracts in San Francisco. According to this interpretation, trade cities are characterized by a more highly mobile population than are manufacturing cities. The second interpretation refers to the comparative profiles of socio-economic rank in trade and manufacturing cities, according to which the growth of industrial cities has tended to weld the urban status structure into a socio-economic continuum, reflected in residential distribution.²³ It is assumed that in trade cities the status structure is not organized into quite such a smooth hierarchical arrangement, but instead tends to reveal a two-class system. If this difference in social structure is reflected in the ecological pattern, trade cities would be expected to have a greater proportion than manufacturing cities of their census tracts classified as deviant. There would be more discontinuity in social structure in trade cities which would be reflected in profiles of socio-economic rank.

In any event, the application of the Farber-Osoinach index to trade and manufacturing cities seems to be useful in the investigation of the effects of functions of cities on their ecological patterning on the dimension of socio-economic rank. Further study should provide additional information regarding the relative extent to which residential mobility and socio-economic profile are responsible for the difference in the ecological structure of trade and manufacturing cities.

DISCUSSION

The ability of the index described in this paper to discriminate between tracts of high and low urbanization on the Shevsky-Bell in-

dex, tracts of high and low residential mobility, and between trade and manufacturing cities suggests the existence of a unidimensional scale in terms of the General Pattern and other scales which deviate from this pattern. By the same token, the results concerning tracts deviating from this General Pattern cast some doubt on attempts to place all census tracts on a single continuum of socio-economic rank. To be sure, there is general agreement between the Farber-Osoinach and Shevsky-Bell scores for census tracts in the San Francisco area. This is true whether the tracts are classified as General or Deviant.

A question is raised as to whether or not all tracts of a given social rank on the Shevsky-Bell scale have a comparable desirability and esteem value for all residents of the city. Probably, Shevsky and Bell would consider the desirability of a census tract as a place of residence to be not merely a function of social rank, but also of ethnic composition and degree of urbanization.²⁴ This would imply that the significance of social rank (or economic status) of an area is influenced by its degree of urbanization and ethnic status. This variation in significance would suggest that social rank (or economic status) is not unidimensional in a *social* sense; it may have value only as an operational construct.

However, by regarding socio-economic rank as a dimension for predicting life-style, as in the Farber-Osoinach index, it is implied that criteria may exist in the general population for evaluating areas.²⁵ It is further implied that the life-styles prevalent in tracts deviating from the General Pattern of socio-economic rank are founded on different perspectives than those in the General Pattern. Possibly, the understanding of the influence of socio-economic rank on urban life may be increased if deviance in socio-economic status is viewed in terms of dimensions of life-style coordinate with the General Pattern, rather than by introducing additional concepts such as urbanization and ethnic status to modify the meaning of social rank. Indeed, a number of questions may be raised about different kinds of social structure of

²³ The problem of continuous *versus* discontinuous status systems in manufacturing and trade cities is discussed more fully in Osoinach, *op. cit.*

²⁴ Shevsky and Bell, *op. cit.*, p. 19.

²⁵ Cf. Rossi, *op. cit.*, pp. 163-172.

census tracts possibly indicated by the various patterns of scores which deviate from the General Pattern of socio-economic rank.

Because of the differences found in the number of tracts falling into the General Pattern in trade and manufacturing cities, other questions are in order concerning the adequacy of indices of socio-economic rank which are not at the same time primarily

homogeneity indices. Indices which fail to deal directly with the homogeneity factor, perhaps, are not equally appropriate to all cities.

Finally, in the evaluation of indices of socio-economic rank, there is the important question: are the time and labor spent in applying those indices which require highly complex computations always justified?

THE OCCUPATIONAL VARIABLE AS A RESEARCH CATEGORY *

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The degree of precision and stability of the concept of occupation is examined. Research suggests that volunteered statements by subjects of their occupations are highly colored by status conceptions. Data further suggest that occupation varies over time. Gainful worker and labor force concepts of the Census are evaluated in the light of these considerations. Finally, the special need for attention to part-time jobs and multiple job holding is pointed out. To overcome some of the problems of job changing, the use of broad categories is suggested. To increase precision, it is suggested that data refer to actual work activities over a specified period, even though more than one work activity is carried out, rather than permitting respondents to choose and name their occupations.

IN present-day, Western society, occupation has become a fundamental index of status and a standard of self-respect. A hundred years ago, in rural America, when two strangers met, the first question was likely to be: "Where are you from?" References to the "hill country" or other place of origin could be reliably translated into judgments concerning probable wealth or previous experience. If it chanced that the two were from the same area, the next question was apt to be about the other's name. In a period of stable residence, when one family lived on the same farm for several generations, a name gave a reputation to those who bore it and those who knew the name knew the reputation. But under modern conditions of mobility, migration, the reduction in size of family, and urbanization, neither place of origin nor name is likely to tell one much about the man. Instead, one asks of a stranger: "What do you do?"—and the

other will understand that the "what" refers to the other's occupation. Depending on his answer, the questioner can estimate the size of the man's income, where he lives, where he works, the number of his children, how he and his family spend their leisure time, what clubs he belongs to. And from such estimates the questioner can in turn make a judgment as to how he should behave toward the respondent—whether to accord him respect, indifference, or contempt, whether to seek his help or offer him help, whether he wants him as a friend. As origin and name have become unreliable as standards of judgment, occupation becomes that much more important. In addition, in traditional societies, occupation by itself is hardly a status indicator at all. What a man does for a living is largely ascribed and is often determined at birth, and since the range of possible choices is small in most such societies, occupational variation will be comparatively slight. By contrast, the range of occupations is very great in modern society, and what a man does is often a matter of achievement. Occupation then becomes a factor which con-

* Considerably revised version of a paper read at the annual meeting of the Rural Sociological Society, August, 1958.

fers status in its own right, in addition to such other factors as age, sex, marital status, and race.

THE ASSUMPTION OF OCCUPATIONAL STABILITY

It might be claimed that this rise to a central position of "occupation" in modern society itself justifies the emphasis given to it by sociologists in their research: one can hardly imagine a questionnaire or interview schedule which excludes reference to the respondent's or his parent's occupation. Yet students of society need not adopt its biases; nor should they. And one of the easiest ways of adopting biases is by identifying "sociological" with "social": the framework we use for studying a society should hardly be the customs of that society. That members of our society make inferences about a man's status from his occupation is itself a social fact of importance. We may study the reliability of such inferences. But the concepts we use for such studies are not necessarily the layman's. A scientific concept must meet other criteria, including precision and stability.

In everyday parlance, references to a man's occupation usually presume something stable about the man. We have, in fact, no terms in everyday language referring to partial or shifting occupational involvements except the invidious "part-time" or "temporary." For example, the term "part-time farmer" carries with it a half-apologetic connotation, suggesting a man whose primary involvement is elsewhere but who has a few acres in which he likes to putter about. We do not want part-time brain surgeons to operate on us; during President Eisenhower's last serious illness perhaps the worst label his enemies could apply to him was "part-time president."

These uses present no serious problem in social investigation. Unfortunately, however, the association of "part-time" with casualness has been taken over by and infuses the thinking of census-takers and researchers. For example, The Sixth International Conference of Labour Statisticians presents the following definition of the term "occupation" for universal application: "the trade, profession or type of work performed by the indi-

vidual. . . ."¹ This definition does not say "trade or trades," but only *the* trade, tacitly assuming that each person will have only one. More fully, Ann Roe, after defining occupation as "whatever an adult spends most of his time doing" continues:

Being a housewife, in this sense, is an occupation; so is being a mother. Being a father is not an occupation in this sense because it almost never happens that it occupies the major part of a man's time, or that it is the central focus of his activities. Stamp-collecting can be an occupation, and so can following the races. The occupation, then, is the major focus of a person's activities, and usually of his thoughts.²

Roe's definition includes more than most. Yet if her definition is restricted to work or economic activities the result is the usage of most researchers. An individual is allowed only one occupation on most questionnaires, a fact that has important consequences for research since occupation is so often used as the predictor of other behavioral characteristics, and is the single index most commonly used to define social status and social class. If a person has two occupations, then the researcher is faced with the problem of assigning him two positions; if the respondent is restricted to one occupation, then the results will vary depending on which occupation the respondent offers.

Such an assumption about occupational stability may be properly defensible in the case of Japanese factory workers, of whom Abegglen writes:

Membership in the Japanese productive group is a permanent and irrevocable membership. Workers at all levels of the factory customarily work in but one company. They spend their entire career in that single firm which is entered immediately on completing their education. The firm will continue to provide the worker's income at whatever disadvantage to itself, and the worker will continue in the company's employ despite possible advantage in moving to another firm.³

¹ International Labour Office Studies and Reports, New Series, No. 7 (part 4), *The Sixth International Conference of Labour Statisticians*, Resolution I, paragraph 14, Geneva, 1948, p. 54.

² Ann Roe, *The Psychology of Occupations*, New York: Wiley, 1956, p. 3.

³ James G. Abegglen, *The Japanese Factory: Aspects of Its Social Organization*, Glencoe, Ill.: Free Press, 1958, p. 128.

Is this assumption of stability defensible in the case of American workers? If it is not, then it does not belong in a science that hopes for universal generalizations. The ideology of the free market views the worker as moving freely between jobs in response to the pull of economic opportunity, a conception at the opposite pole from the case of Japanese industry. But this view is still an assumption; its mere assertion can not assess the adequacy of the assumption of occupational stability. The data on this subject are insufficient, but their direction is clear, as the findings of the studies discussed below indicate.

Miller and Form,⁴ for example, in a study of 276 work histories of persons in Ohio, tried to characterize career patterns by dividing careers into initial, trial, and stable periods. They then asked what proportion of persons in each occupation achieved a stable period. In spite of their use of a very liberal definition of stability—three years on a job—they found a range from a high of 88 per cent of professionals to a low of only 24 per cent of unskilled workers who achieved a stable work period during their lives.⁵ Cohort analysis was used in an investigation of six cities by Gladys L. Palmer⁶ in order to estimate the proportions remaining in major occupational groups for the whole of their working careers. The approximate results are as follows:

Professional, technical, and kindred workers	1/3
Managers, officials, and proprietors, except farm	1/4
Clerical and sales	1/5
Craftsmen, foremen, and kindred workers	1/3
Operatives and kindred workers	1/5
Service workers	few
Laborers, except farm and mine	few

Consistent with these findings is the difficulty experienced by researchers in attempting to

predict "present job" from first job or earlier jobs.⁷ Finally, a study by Lipset and Bendix⁸ of 935 principal wage-earners in Oakland, California presents data on the per cent of work life spent in jobs other than those in the respondent's present occupational group as follows:

Present Occupational Group	Per Cent of Time in Other than Present Occupational Group
Professionals	19.8
Semi-professionals	30.8
Own Business	58.5
Upper white-collar	61.4
Lower white-collar	52.4
Sales	52.0
Skilled	44.1
Semiskilled	47.5
Unskilled	55.8

Here, professionals and semi-professionals stand out as striking exceptions to the tendency of principal wage-earners to spend over 40 per cent, and in most cases over one-half, of their working lives in occupation groups different than their present one. These results are the more impressive because these figures are limited to males 31 years of age or over—so that summer jobs, jobs while at school, and the like are largely eliminated, the data referring to mature persons whose careers are well underway. Lipset and Bendix characterize job-shifting in another way by counting the number of jobs held:⁹ their subjects

⁷ See, e.g., Seymour M. Lipset and F. Theodore Malm, "First Jobs and Career Patterns," *American Journal of Economics and Sociology*, 14 (April, 1955), pp. 247-261; and Jaffe and Carleton, *op. cit.*, p. 35.

⁸ Seymour M. Lipset and Reinhard Bendix, "Social Mobility and Occupational Career Patterns. I. Stability of Jobholding," *American Journal of Sociology*, 57 (January, 1952) pp. 366-374.

⁹ In passing, it is worth observing that a considerable obstacle to research in career patterns is the lack of adequate measures of mobility. See Charles A. Myers, "Patterns of Labor Mobility," in William Haber et al., editors, *Manpower in the United States: Problems and Policies*, New York: Harper, 1954, pp. 154-165. For a sophisticated attack on the measuring problem, see Isadore Blumen, Marvin Kogan, and Phillip J. McCarthy, *The Industrial Mobility of Labor as a Probability Process*, Ithaca, New York: Cornell Studies in Industrial and Labor Relations, VI, 1955, wherein a probability model is used for describing the movement of workers between industrial groups; the data are from the

⁴ Delbert C. Miller and William H. Form, "Measuring Patterns of Occupational Security," *Sociometry*, 10 (November, 1947), pp. 362-375.

⁵ Cf. Percy E. Davidson and Dewey Anderson, *Occupational Mobility in an American Community*, Stanford: Stanford University Press, 1937, esp. p. 81.

⁶ Gladys L. Palmer, *Labor Mobility in Six Cities*, New York: Social Science Research Council, 1954. The cohort analysis itself was made by A. J. Jaffe and R. O. Carleton in *Occupational Mobility in the United States, 1930-1960*, New York: King's Crown Press, 1954, pp. 53-57.

reported a total of 4,530 such jobs, averaging 4.8 per man for 25.3 years in the labor force.¹⁰ In view of data such as these, it is not surprising that Jaffe and Carleton conclude that: "Change of occupation or job is characteristic of the American man."¹¹

THE LABOR FORCE THEORY

Unfortunately, few data on this problem are provided by the most comprehensive occupational source of all, the U. S. Census.¹² This is due in considerable part to the use of the labor force concept.¹³ Certain results of this usage are relevant to the present discussion.

Most countries that collect statistics have made use of the gainful worker concept,¹⁴

Bureau of Old Age and Survivors Insurance. See also Herbert S. Parnes, *Research on Labor Mobility: An Appraisal of Research Findings in the United States*, New York: Social Science Research Council, 1954, Chapter 2; and Edward Gross, *Work and Society*, New York: Crowell, 1958, pp. 167-172.

¹⁰ This conclusion is restricted only to actual jobs held and ignores any unemployment or movement out of the labor force altogether between jobs. If these experiences are taken into account, then the average number of "positions" is found to be 7.4. In comparison with Lipset and Bendix' figures, Gladys L. Palmer's study of six cities reports an average of 2.6 jobs per person over a ten year period. If it were legitimate to convert this to a 25.3-year base (to make it comparable with the Lipset-Bendix average), then Palmer's average number of jobs per man is 6.6, a figure undoubtedly inflated by the particular 10-year period she used (1940 to 1950). See Palmer, *op. cit.*, p. 72.

¹¹ Jaffe and R. O. Carleton, *op. cit.*, p. 49. Two excellent recent case studies of the career mobility process are Margot Jefferys, *Mobility in the Labour Market: Employment Changes in Battersea and Dagenham*, London: Routledge and Kegan Paul, 1954; and Leonard P. Adams and Robert L. Aronson, *Workers and Industrial Change: A Case Study of Labor Mobility*, Ithaca, N. Y.: Cornell University Press, 1957.

¹² See "Multiple Jobholding: July, 1957," *Current Population Reports: Labor Force*, Series P-50, No. 80, Washington, D. C.: Bureau of the Census, February, 1958. See also Series P-50, Nos. 30 and 74.

¹³ A brief but authoritative discussion is John D. Durand, "Development of the Labor Force Concept, 1930-40," in Louis J. Ducoff and Margaret Jarman Hagood, *Labor Force Definition and Measurement*, New York: Social Science Research Council (no date), Appendix A, pp. 80-90. See also Philip M. Hauser, "The Labor Force as a Field of Interest for the Sociologist," *American Sociological Review*, 16 (August, 1951), pp. 530-538.

¹⁴ See League of Nations, "Statistics of the Gain-

ful worker concept, the enumerator (or Census analyst) attempts to place the man, based on his answers to certain questions. The Monthly Labor Force Report (MLFR) ¹⁵ poses as its major question, "Last week what was this person's main activity?" The alternatives allowed are "working," "looking for work," "keeping house," "going to school," or "something else." If the respondent reports that he was working or looking for work at some time during the week, he is assigned to the labor force; if he declares that he was keeping house or going to school, he is not so classified. But being *in* the labor force is given precedence over being *out* of it. For example, if a respondent should say that he was going to school but in answer to a later question reply that he did *some* work for pay or profit, he is classified as being in the labor force.¹⁶ To be "unemployed," a person actually must be looking for work, unless prevented from doing so by temporary illness or a belief that no employment is available in the community or in his line of work. Here too if he does *any* work for pay or profit, he is placed in

fully Occupied Population: Definitions and Classifications Recommended by the Committee of Statistical Experts," *Studies and Reports on Statistical Methods*, 1, Geneva, 1938.

¹⁵ The other major organization collecting current statistics, the Bureau of Labor Statistics, however, makes use of establishment data.

¹⁶ Included also are persons working for a relative without pay if they worked 15 or more hours during the week.

the labor force and in the occupation in which he has worked previously.

These procedures are not based on expediency; nor are they arbitrary or a reflection of the need for the Census to be all-inclusive. They are based on a definite theory of labor and work orientation which may be characterized in terms of three elements.¹⁷ The first is the concept of labor market attachment: a person who is so attached in the sense of having a job or looking for one is part of the labor force, whether or not he is in fact working. Consequently, persons who presumably would have been working except for personal reasons (temporary illness, vacation, attending to personal affairs), economic reasons (labor disputes), bad weather, expectation of a job in the future, or the like are classified as being in the labor force and as employed¹⁸ since they are not competing with others for the available jobs. In turn, a person looking for work is putting pressure on the market to provide him with a job and therefore has an "attachment" to the labor market, but he is unemployed. This emphasis of the MLFR on labor market attachment means that the intention is primarily to measure unemployment, rather than employment. Whatever occupation the individual happens to be in is considered secondary to the question of whether he is in any way attached to the labor market; if attached, the question becomes one of simply deciding whether or not he is actively looking for work (if at all possible), in which case he is "unemployed."

The second element of the theory follows directly from the first: the labor force concept is a rejection of the labor input concept. A person is assigned to the labor force whether he works or not. This feature of the theory seriously affects the securing of Census data on part-time farming and part-time occupations in general because it precludes any interest in *how much* work a person does

and focusses upon whether he does any at all. This focus again is no accident but is explicitly based on an assumption about the society described by Jaffe and Stewart:

In an economy such as that of the United States and other developed areas of the world, industry is organized so as to tend generally to employ persons full time, or not at all. Hence, if a person is working he tends to work full time, that is, 40 or more hours a week. On the other hand, during the weeks in which he is not working full time, whether for voluntary or involuntary reasons, he tends not to work at all.¹⁹

These authors imply there is little point in learning how much time a person gives to a particular occupation since it is assumed that if he works at all it must be 40 or more hours a week. Jaffe and Stewart point out that this assumption does not hold for some Latin American and "peasant" countries, where the labor force procedures would not provide adequate information.²⁰ It should be noted, however, that in rural counties, rural-urban fringes, suburban areas, and in general for part-time occupations, although persons may work 40 hours or more *altogether* in the course of a week, they do not necessarily work 40 or more hours a week in the occupation they report to the Census. If a person reports two jobs, the one at which he spent more hours is recorded as his job for the week. Moreover, any procedure permitting only one occupation per man passes over not only the part-time farmer (who is placed in the category "farmer") but all persons whose occupations are, by definition, seasonal or involve a continuous shifting from one job to another.²¹ This problem is discussed further below.

¹⁹ Abram J. Jaffe and Charles D. Stewart, *Manpower Resources and Utilization: Principles of Working Force Analysis*, New York: Wiley, 1951, p. 55.

²⁰ The authors dramatize the undesirability of using labor force procedures by citing the inadequacy of working population data for two countries that do use such methods—Puerto Rico and Japan. *Ibid.*, Appendix G, pp. 469-476.

²¹ A remarkable by-product of the usual tendency to pass over job-shifting is provided by the effects of short-term shifts—weekly or seasonal—on income data. Herman P. Miller shows that the pronounced skewness of the income curve is due not only to actual inequities in income distribution but also to the artifact created by combining the lower earnings of part-time and part-year workers

¹⁷ This theory is rooted primarily in economics rather than in sociology. See John D. Durand, *The Labor Force in the United States, 1890-1960*, New York: Social Science Research Council, 1948.

¹⁸ Since 1957, persons on layoff who said that they had definite instructions to return to work within 30 days of the date of layoff are classified as unemployed, and persons waiting to report to new wage and salary jobs scheduled to start within the following 30 days are classified either as unemployed, or, if in school, as not in the labor force.

The third element in the theory of the labor force follows from the rejection of the labor input concept. If the search is not for the contribution to the national product but to determine whether persons were "working" or "seeking work," then the latter question must be left to the individual, for this is an attitudinal matter. This problem is also present when the gainful worker concept is used; but the labor force concept does not avoid it.

SOME PROBLEMS IN OCCUPATIONAL CLASSIFICATION

The attitudinal problem is especially serious in the case of women since they are likely to be reported as working in terms of the cultural definitions of appropriate work for their sex. For example, Hauser notes that in France, all farmers' wives are included in the labor force but in Sweden they are excluded categorically. In the Philippine Census of 1939, women doing housework were combined with domestic servants as part of the working force; in the United States, we exclude housewives, but include domestic servants; in Mexico, in 1940, neither housewives nor servants were included. How should one classify the woman who will take a job if one comes along, but otherwise does not conceive of herself either as working or looking for work?²² Equally serious is the attitudinal problem in the case of the unemployed: they must be looking for work, a situation that implies a belief that there is work to be obtained.

The attitudinal problem raised by U. S. Census classification procedure is quite general.²³ For most studies draw their conclusions from occupational data on a questionnaire item which simply asks for "Occupation," leaving it to the respondent to decide how he thinks of himself.

with those of full-time and full-year workers. See his *Income of the American People*, New York: Wiley, 1955, Chapter 3.

²² Philip M. Hauser, "Mobility in Labor Force Participation," in *Labor Mobility and Economic Opportunity*, New York: Wiley, 1954, pp. 8-46. See also Nathan L. Whetten, *Rural Mexico*, Chicago: University of Chicago Press, 1948, pp. 64-65.

²³ One of the most thorough discussions of the attitudinal problem in working force analysis is provided by Jaffe and Stewart, *op. cit.*, Chapters 18-21 and Appendix E.

When occupation is being used as an index of social class, the problem of self-conceptions of working women is severe. If it is considered appropriate for a woman of a particular class to be working at all she may report on occupation, but if her social class frowns on such employment she may report herself as a housewife.

In studies of occupational choice, the question is asked: "What occupation would you like to (or do you expect to) enter?" Does the respondent interpret this to mean the first job he hopes or expects to hold (believing that this will be his lifetime job), or the job he thinks will occupy most of his working life, or the job he considers worthy of him? The term "occupation" itself has different meanings for persons of different status and in different parts of the social structure. To one of Spiro's *kibbutzniks*, his job: "... becomes more than a way of making a living. It becomes a *sacred task*, a calling, in the religious sense of that term, dedicated, not to the greater glory of God, but to the welfare of his group."²⁴ Such an attitude is shared by professionals universally, at least in attenuated form.²⁵ But many workers in less prestigious occupations take a wholly casual attitude to the job, moving on to another if it seems to offer more money or prestige. Moreover, job histories—for example, whether or not an individual lists a particular job—are influenced by the impact of the type of work on personality.²⁶

The attitudinal problem is as great for intergenerational as for career mobility studies. One of the best of such studies—Rogoff's investigation of Marion County, Indiana—presents data provided on a marriage license application by a man for himself and for his

²⁴ Melford E. Spiro, *Kibbutz: Venture in Utopia*, Cambridge: Harvard University Press, 1956, p. 89.

²⁵ See my discussion of the criteria of professionalism in "Some Suggestions for the Legitimation of Industrial Studies in Sociology," *Social Forces*, 33 (March 1955), pp. 233-239.

²⁶ On the extent of vocational identification of semi-skilled and skilled workers, see E. Wight Bakke, *Citizens Without Work*, New Haven: Yale University Press, 1940, p. 96, and *The Unemployed Man*, New York: Dutton, 1934, pp. 68-70 and 256-261. See also Howard S. Becker and James W. Carper, "The Development of Identification with an Occupation," *American Journal of Sociology*, 61 (January, 1956), pp. 289-298.

father.²⁷ If a man gives as his own occupation "department store clerk" and as his father's "store proprietor," what inferences may we draw? Rogoff (necessarily) accepting the reports as they stand, would count this as one case of the son of a store proprietor who became a department store clerk. But what might these statements mean? If the applicant had been strictly following U. S. Census MRLF procedures, he would mean: "Last week I spent the majority of my working hours as a department store clerk, and during the same week my father spent the majority of his working hours tending to his store." Obviously, the replies could mean something very different; for example: "Last week I spent most of my working hours as a sign painter, but I was just filling in between jobs since it is the slack season down at the department store where I usually work as clerk. My dad was probably tending his store, though my mother has had to take over most of the work since my dad's heart attack." Or: "I didn't work at all this last week and haven't for some time since this recession hit, but I expect the store where I usually clerk to take me back soon. My dad owns a store except Safeway's just about put him out of business." Or: "I work as a department store clerk a full 40-hour week, but with the cost of living being what it is today, my wife and I have bought this small piece of land where we can keep a cow and grow some of our own vegetables. If they lay me off at the store, I still have something. My dad runs the small gas station on the edge of the farm by the road. We get a few pennies from that and it means a lot to dad to be able to put the key into the lock every morning." Each of these statements about "father's occupation" involves a conceptualizing or "freezing" process in the son's thinking: the response reflects his point of view toward his father's career chances. If the father is temporarily working at Safeway, for example, but has owned his own store and expects to own it again soon, the son may refer to him as an independent businessman; but, if he believes his father will never make it, he may call him a clerk. The stage of the father's career which the son is describing is also important. Some occupa-

tions involve early peaks (as in the case of airplane pilots or professional athletes) and a man's father may be "descending" to a selling or clerking job by the time his son fills out a marriage license application; in others, the father may be about to move from, say, a white-collar to an executive job. Clearly, there are many such possibilities. Asking a person to give his occupation is no simple question. We are really asking him: "Who are you and how do you conceive of yourself in relation to your fellows?"

One of the most important, although neglected, forms that occupational variation takes is that of the widely prevalent dual occupation. As noted above, this job situation is largely ignored by the U. S. Census,²⁸ the individual being assigned to the occupation in which he spent most of his working hours during the previous week. Yet the pattern of two occupations in a year is the usual one for seasonal workers, and many others hold two jobs on a regular basis.²⁹ Some occupations, in fact, require a second. This is usually true of illegal occupations, at least for income tax purposes, and is often the case in the arts and teaching, in which love of the work may keep a person in the occupation while low income forces him to carry a second job. The importance to many part-time farmers of life in the country and other values associated with rural living³⁰ does not gainsay the fact that in order to earn an adequate living they too must often hold a second job in a nearby city.

Again, consider the case of Massachusetts state legislators, reported by MacRae. At the time of his study, their pay was 4500 dollars per session, about which MacRae comments: "Although this compares favorably with the salaries of legislators in other states, it is probably not enough to support a representative at the standard of living that would normally go with the status of his

²⁸ In the sense of dealing with it along with regular data. Special reports are issued separately on multiple jobholding from time to time, as cited in footnote 12.

²⁹ "Multiple Jobholding: July, 1957," *op. cit.*, reports that 5 per cent of the total employed or an estimated 3,500,000 persons were multiple jobholders during the week ending July 13, 1957.

³⁰ See, e.g., Paul H. Landis, *Rural Life in Process*, New York: McGraw-Hill, 1948, Chapter 17, esp. p. 249.

²⁷ Natalie Rogoff, *Recent Trends in Occupational Mobility*, Glencoe, Ill.: Free Press, 1953.

office. . . ."³¹ The problem here is not simply that of income but also of meeting community expectations of how a legislator should live. Here then is the well known paradox of a community deciding upon a low salary while insisting upon a relatively expensive style of life. Such pressure is an invitation to corrupt practices—or to a second job. MacRae found that Massachusetts legislators chose the latter, reporting that 26 per cent of the 1951–52 membership of the House were lawyers, 25 per cent heads of businesses, 13 per cent professionals or semiprofessionals, and 10 per cent in insurance or real estate. These occupations are of such a nature that they can be carried on simultaneously with their political duties. (Here is a powerful selective process in the recruitment of persons into political careers, for individuals who are employees of others would find it very difficult to combine such jobs with legislative duties.)³²

SOME PROPOSALS

The solution of the problem of developing stable, valid occupational categories is not yet clear, but the questions raised in the foregoing discussion suggest certain lines of attack. We are studying *work*, something that appears to be a rapidly shifting activity, at least in the United States, and seek categories for describing it. At the same time, categories that are adequate for shifting activity will also be applicable to stable activity, although the reverse is not true. Herbert S. Parnes, in a discussion of the measurement of occupational mobility, points out

³¹ Duncan MacRae, Jr., "The Role of the State Legislator in Massachusetts," *American Sociological Review*, 19 (April, 1954), pp. 185–194; quotation, p. 185.

³² Another case is reported by Arensberg, who describes a New England plastics industry which was trying to refill some of the abandoned textile mills. In this case, a low-wage highly seasonal industry was able to obtain a foothold in the market because of the part-time farming pattern in the local community, the members of which were eager to turn to farming both seasonally and cyclically (in case of layoffs) and thereby increase their earnings. Industry needed them quite as much as they needed industry. See Conrad M. Arensberg, "Industry and the Community," *American Journal of Sociology*, 48 (July, 1942), pp. 1–12; see esp. pp. 4–5.

that the narrower the occupational categories used the more mobility is possible.³³ For example, a study which distinguishes between punch press and drill press operators will "catch" the case of the man who shifts from one to the other, whereas a study which uses only "semi-skilled worker" may miss it. On the other hand, the finer the divisions the greater the likelihood that a man in one occupation will later be found in a different one. Therefore, those fairly broad categories within which respondents can "move around" without thereby altering position in the overall occupational scale are required for *stability*.

Yet precision of the referent of the occupational category is the fundamental research need. We may study what "proprietor" means to a man who calls himself that, but first we must identify his actual work. And since there is considerable shifting of work, we will need to specify a *time* period, as the labor force concept does. But the length of this time period rests on the degree of job-shifting typical for the region or type of status being investigated. In an isolated rural farm area, the span might be a month; in a rural-urban fringe, or in a factory area, perhaps a week. The range of such shifting also varies by age and sex. To determine the length of time period therefore clearly requires preliminary research. Probably a series of time periods—specifying the work of the respondent last week, two weeks ago, three weeks ago, and so on—would be most useful.

Dual occupations—whether carried on simultaneously (that is, in the same day) or seasonally—should not present special difficulties if they are recognized as an established occupational pattern. If an individual spends all of his working time, for a specified period, at one activity, as does the average physician in mid-career, he would be recorded, say, as "physician." Another man who spent part of his working time, over the specified period, as a waiter and part as a farm laborer would be recorded as a "waiter-farm laborer." There is no theoretically intrinsic reason for pushing everyone into "one and no more than one" occupation. Nor is the possibility that the dual category would

³³ Parnes, *op. cit.*, p. 27.

seem "strange" to the layman an important consideration.³⁴

The respondent should not be permitted to "choose" the occupation he would like to report. Indeed, in view of the significance of the attitudinal component in the respondent's identification of his occupation, it seems certain that some of the frequently reported relationship between occupation and social class is spurious. The analyst, of course, may do what he likes with the data. If he wishes to call all of his "waiter-farm laborers" waiters, then he may do so—if he can explain why. But it is *he* who must conceptualize or categorize the data, not the respondent. As noted above, some activities do not engage a person's full working time, for example, the work of newsboys or legislators. Again, if the analyst wishes to treat these activities in the same way as (that is, assign them equal weight with) full-time occupations, he may do so. Whatever he does, however, should rest on reliable data that describe actual work activities.

These considerations imply certain changes in the labor force procedures of the Census. The initial question could remain the same: "Last week what was this person's main activity?" This statement contains a specific time referent, although whether or not the time should be a week should be re-examined. For a national survey, a fixed time is desirable, but individual researchers should check the suitability of the period for their particular subjects and areas. The alternatives—working, looking for work, keeping house, going to school or something else—could also be retained. Those working or looking for work would be assigned to the labor force, as they are at present, and the existing criteria for distinguishing between the employed and unemployed might also be retained. However, whether classified as em-

ployed or unemployed, before being assigned an occupation, the individual would need actually to have worked during the time period. This means that the "inactive employed," now classified as "employed," would not be assigned an occupation. At present, this category contains the largest percentage of those who name their occupations with little restriction and, probably, are most influenced by attitudinal considerations.³⁵ The same reasoning applies to the "inactive unemployed," which includes persons who were not job-hunting because they believed that there were no jobs to be had in their specialty in their communities. Again, their point of view determines their occupations.

After discovering whether or not a person was working last week, the next step would be to ascertain the type of work performed and how much of the time period he engaged in it. If a person reported fulltime work as a salesman; or if he worked mornings only; or if he worked afternoons as a store proprietor—whichever the case, it would be recorded. Some appropriate fractional unit could be worked out.³⁶

This procedure, it should be emphasized, does not involve giving up the labor force concept (with its notion of labor market attachment) in favor of the labor input concept. But it provides the analyst with raw data that are much closer to actual activities than do present procedures. From such data he could combine and reassemble in accord with either concept.³⁷ In this manner, the

³⁵ The category includes persons who might have been working except for "personal reasons" (temporary illness, vacation, attending to personal affairs), economic reasons (labor disputes), bad weather, or expectation of a job in the future. But see the special cases noted in footnote 18.

³⁶ In our society, we might define the work day as a maximum of 16 hours, with two "work-time units" of 8 hours each. A factory worker who worked a 40-hour week over five days would have completed 5 work-time units. If he worked three 8-hour days at the factory and worked as a gardener (for pay) on Saturday afternoon, he might be recorded as having worked three work-time units as factory worker and one-half work-time unit as a gardener.

³⁷ For the illustration in the previous footnote, application of the labor force concept, for either case, would, result in the single category of "factory worker." Use of the labor input concept would differentiate them in accord with the number of hours worked during the two work activities.

³⁴ Perhaps it would appear less strange to the layman than to the analyst himself, a professional. The latter experiences the least occupational shifting in his career. In contrast, Lipset and Bendix find so much shifting back and forth between manual work and owning one's own business that they write: "It is our guess that the creed of 'individual enterpriser' has become by and large a working-class preoccupation." Seymour M. Lipset and Reinhard Bendix, "Social Mobility and Occupational Career Patterns. II. Social Mobility," *American Journal of Sociology*, 57 (March, 1952), p. 502.

researcher would not be bound by assumptions of the Census which he may not share, any more than the investigator who gathers his own data.

For most social analysts, this discussion suggests a similar expansion of questions. Instead of a single item—"Occupation"—they need a series of items asking the respondent to describe the nature of his work over a specified period and the amount of time spent at different types of work. From such raw data, highly useful occupational data may be derived.

A final question concerns the financial costs of incorporating the suggested procedures into research inquiries and census counts. Because more questions would be asked, coding would be complicated and costs would be higher. But the problem is less a question of absolute increase in cost than whether or not this expenditure produces results of greater value than the increment—a proper subject for research and thoughtful consideration. In some studies, the interest in occupation may be only secondary and many problems may be avoided by using broad categories. But in investigations in which occupation is an independent variable intended to predict something else, the risk of low precision or instability may be serious. In a longitudinal study, for example, it would be hazardous to report apparent changes in the behavior of, say, "proprietors" without considering the possibility that many of them are no longer qualified for this designation at the time the changes take place. On the other hand, a similar study of professionals, who do not seem to shift occupations very much, might safely ignore the question. In studies of women workers and in inquiries concerning attitudes having status implications, the magnitude of self-judgments in the respondents' statements of occupations should be estimated and if it appears to be large, the findings may be considered questionable.

If a researcher contemplates the use of

occupational data gathered by others, the expense of gathering his own data may be assessed in the light of the scientific costs of accepting the assumptions implicit in their categorization. Use of Census occupational data tacitly involves two types of assumption: first, the subjects' identifications of their occupations are assumed to be descriptions of their actual work; and, second, the assumptions, discussed above, involved in the labor force concept are accepted. Neither of these may be in accord with the assumptions of the investigator himself. A similar point holds for the widely-used North-Hatt scale, which may be less a prestige scale of types of work than of the relative prestige of certain terms used in American society. Thus Hughes writes:

The names [of occupations] are tags, a combination of price tag and calling card. One has only to hear casual conversation to sense how important these tags are. Hear a salesman, who has just been asked what he does, reply, "I am in sales work," or "I am in promotional work," not "I sell skillets." School teachers sometimes turn school-teaching into educational work, and the disciplining of youngsters and chaperoning into personnel work. Teaching Sunday School becomes religious education, and the Y.M.C.A. Secretary is "in group work."³⁸

Those using the North-Hatt scale necessarily restrict themselves to occupations which, like "public careers" in Dubin's usage,³⁹ are sufficiently accessible to public scrutiny to be rated by a mass sample. Since many studies are not concerned with prestige or with other attitudes but rather with actual types of work, whether widely known or not, in such cases the value of valid data would appear to be worth their cost.

³⁸ Everett C. Hughes, "Work and the Self," in J. H. Rohrer and M. Sherif, editors, *Social Psychology at the Crossroads*, New York: Harper, 1951, pp. 313-314.

³⁹ Robert Dubin, *The World of Work*, Englewood Cliffs, N. J.: Prentice-Hall, 1958, pp. 276-277.

SOME FACTORS IN A PROBABILITY SAMPLE SURVEY OF A METROPOLITAN COMMUNITY *

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Surveys conducted by the University of Michigan's Detroit Area Study in 1956, 1957, and 1958 employed an area-probability sample of metropolitan Detroit adults. No substitutions were permitted, nor was a specific limit put on the possible number of call-backs. To achieve the 2,313 interviews necessary for an 87 per cent response rate, a total of 7,743 calls was required. Although three-quarters of the completed interviews were obtained after three calls, there is evidence that biases in the sample would have resulted had the interviewing stopped after two call-backs. Available data indicate that the amount of dissimilarity between respondents and nonrespondents is probably not large enough to influence the representativeness of the completed sample, given the response rate of these surveys.

FEW students of survey research would challenge the claim that sampling techniques based on strict probability models afford far more reliable and valid results than do other types of sampling.¹ To be sure, for some purposes and under some conditions quota samples and "mod-

ified" probability samples may actually be preferable to more rigorously designed research; but even in these situations most survey researchers would prefer to base their analyses on probability models, if costs and time were not considerations.²

A basic charge leveled against quota sampling is that ultimately the interviewer is also the sampler. The interviewer makes the final decision as to which of the many units in the research universe will fall into the sample. He may choose to interview those persons within his quota who are most willing to be respondents and are easiest to locate. It does not necessarily follow that these "volunteers" are a representative sample of all possible units with similar quota characteristics.³ They could, in truth, be representative, but there is no statistically accept-

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¹ One exception to this claim may be found in Francis E. Lowe and Thomas E. McCormick, "Some Survey Sampling Biases," *Public Opinion Quarterly*, 19 (Fall, 1955), pp. 303-315. These authors conclude: "... it remains to be demonstrated that, for ordinary polling purposes, a random sample, in the usual statistical sense, can be drawn with confidence from human populations under a democratic way of life, with prevailing field methods." (p. 315) It has been demonstrated repeatedly that an adequately designed probability sample is capable of producing units which are representative, within known confidence limits, of a given research universe. See, e.g., C. A. Moser and A. Stuart, "An Experimental Study of Quota Sampling," *Journal of the Royal Statistical Society, A*, 116 (1953), pp. 349-405; Leslie Kish, "A Procedure for Objective Respondent Selection Within the Household," *Journal of the American Statistical Association*, 44 (September, 1949), pp. 380-387; Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting*, Chicago: University of Chicago Press, 1954, Appendix C; *A Social Profile of Detroit: 1956*, Ann Arbor: The Detroit Area Study, 1957, Appendix A. See also Morris H. Hansen, William N. Hurwitz, and William G. Madow, *Sample Survey Methods and Theory*, New York: Wiley, 1953, Volume I, Chapter 1.

² W. R. Simmons and others recently made this point during a panel discussion of the question, "Is Probability Sampling Overdone?" at the 1958 meeting of the American Association for Public Opinion Research. Simmons' conclusion was that true probability sampling is underdone. "Proceedings of the Thirteenth Conference," *Public Opinion Quarterly*, 22 (Summer, 1958), p. 208. See also C. A. Moser, *Survey Methods in Social Investigation*, New York: Macmillan, 1958, pp. 106-108.

³ Evidence from an experimental study in England indicates that on many items quota samples do not differ greatly from random samples. On some very crucial items, however, the quota sample is drastically in error. In addition to large biases, the variance of the quota samples in this research were from 150 per cent to about 400 per cent larger than the random sample variance. Thus, in terms of standard error per dollar the quota sample actually may be more expensive than a probability sample. Moser and Stuart, *op. cit.*

able method by which confidence limits for a nonprobability sample can be ascertained.

In a strict probability sample all units in a defined universe are given an equal or known nonzero chance of being selected. The interviewer has no opportunity whatsoever to determine the specific units which he must contact. Unfortunately, the problem of obtaining a representative sample with known confidence intervals is not solved simply by drawing a probability sample. This would be the case only if (1) all units which fell into the original sample yielded a completed interview, or if (2) nonrespondents differed from respondents solely in their failure to grant an interview. The dilemma for probability sampling on most human populations is that neither of these conditions holds.

Survey researchers have grappled with the nonresponse problem for many years.⁴ A considerable body of valuable data is now available which can help in making estimates of costs and in anticipating and correcting biases and weak points in the interviewing stage of a survey. Few studies of response rate which are readily accessible, however, have centered on a large metropolitan area in the United States. Almost all of our information about this important aspect of survey research is based upon studies of relatively small communities or of the entire nation. To an ever increasing extent the metropolitan community is of central interest to the social sciences. In the present paper an attempt is made to describe in some detail the factors which are involved in carrying through a large-scale survey in a metropolitan center with a population of over three and one-half million persons.

THE DATA

Each year since 1952 The University of Michigan's Detroit Area Study⁵ has conducted an annual survey of the adult population of the Detroit metropolitan community.

⁴ Nonresponse is also an important concern of compulsory censuses. See Morris H. Hansen, William N. Hurwitz, and Leon Pritzker, "The Accuracy of Census Results," *American Sociological Review*, 18 (August, 1953), pp. 416-423.

⁵ The Detroit Area Study is associated with the Department of Sociology and the Survey Research Center of the Institute for Social Research at the University of Michigan.

This discussion focuses on the surveys of 1956, 1957, and 1958. The objectives of these surveys required a sample of the adults of greater Detroit which would be representative, within known confidence limits, of all noninstitutionalized adults in the community.⁶

Area-probability samples were constructed for this research which allowed the assignment of specific addresses at which interviews were to be obtained.⁷ No substitutions of any kind were permitted. The interviewer's first task at a designated dwelling unit was to select from all adult residents one person as the respondent. The selection was predetermined for the interviewer, following a method described in the literature.⁸ Again, no substitutions were permissible at this stage of the sample design.

All three surveys used an interview schedule which required an average of one hour and ten minutes to administer. Standard open-ended and closed-answer questions were employed in about equal numbers. The topic of the research, of course, varied each year.⁹

In each of these surveys detailed information was gathered about the interviewing stage. The situation surrounding every contact at every sample address was noted fully by the interviewer. The analyses of these data should present some valuable insights

⁶ The comparability of Detroit Area Study sample distributions with those of the Census attest to the validity of the sample. See *A Social Profile of Detroit: 1956*, loc. cit.

⁷ Detroit Area Study samples were clustered and stratified to meet the needs of the research projects. Detailed descriptions of the specific probability models used in each survey are available from the Detroit Area Study. The sample universe, which is that part of the greater Detroit area that is traced by the Census, includes about 87 per cent of the population of the three-county Detroit Standard Metropolitan Area. The samples were designed by the Sampling Section of the Survey Research Center.

⁸ Kish, *op. cit.*

⁹ In 1955-56 the facilities of the Detroit Area Study were used by Robert C. Angell for research in normative integration, and by Robert Weiss and Robert Kahn for a study of the sociology of work. Daniel Katz and Samuel Eldersveld investigated political behavior in 1956-57. Gerhard Lenski's research on the religious life of greater Detroit took place in 1957-58. Interview schedules used in these surveys are available from the Detroit Area Study. No evidence is available that the content of a specific study influenced that survey's response rate.

TABLE 1. DISTRIBUTION OF CALLS BY NUMBER AND RESULT AT EACH SAMPLE ADDRESS

Result of Call	Percentage Distribution by Number of Calls						Mean No. of Calls
	One	Two	Three	Four	Five or More	Total Sample	
Completed interview	28	36	28	31	25	87	2.6
Refusal or "too busy"	20	16	21	15	18	8	4.6
Respondent not home	20	14	15	16	13	3	6.4
No one home	30	32	34	37	43	1	7.0
Respondent ill, senile	1	1	1	1	1	1	3.2
Language problem	1	1	1	*	*	*	3.7
Total	100	100	100	100	100	100	2.9
Number of cases	2,646	1,888	1,164	767	1,278	2,651	7,743

* Less than 0.5 per cent.

into the processes by which metropolitan samples based on probability models are constructed "call-by-call." As much information as possible was collected on the characteristics of nonrespondents. These data are presented in the final section of the paper. For convenience, the combined samples are discussed below as though they constituted a single survey.

A combination of budgetary considerations and estimates of tolerable confidence limits yielded 2,651 addresses in the original sample which contained eligible respondents. It was determined that a sample of this size should produce the desired number of completed interviews. This decision was predicated on a final response rate of 85 per cent.

No cross-sectional metropolitan survey, of course, can plan on a 100 per cent response rate. Even if every designated respondent were willing to grant an interview, the cost involved in reaching the total sample would be prohibitive. In general, experience has shown that, first, per unit interviewing costs in time and money rise dramatically after a response rate of about 75 per cent is reached, and, second, the distortion in the completed sample due to nonresponse does not seem to be excessive when the proportion of nonresponse is not more than 15 to 18 per cent of all possible interviews. A balancing of these considerations with the desire to achieve perfection in response led to the decision to settle for 85 per cent completion. Nonetheless, any specific response rate figure is unavoidably arbitrary.

RESPONSES AND NONRESPONSES

The 1956-58 Detroit Area Study surveys actually produced 2,313 interviews for a final response rate of 87 per cent (see Table 1). Eighty-seven out of every 100 adults in the sample were interviewed only with the help of an intensive campaign in which the interviewer himself was perhaps the most crucial figure.¹⁰

All sample addresses were sent letters (first-class mail) which heralded the arrival of an interviewer, explained the purpose of the survey, and stressed the fact that no product or service was being sold. The interviewers were also aided by several newspaper articles which described the research and explained the necessity of talking with a certain individual in a certain family. The actual value of these devices in reducing the nonresponse rate is debatable. There is some evidence that their primary effect is that of solidifying the incipient negative or positive attitudes toward an interview held by designated respondents. Most interviewers encourage the use of letters and advance publicity, however, because "they serve as an excellent introduction and ease the initial contact with many respondents."¹¹

¹⁰ For a comprehensive discussion of the role played by the interviewer in the efforts to achieve a low nonresponse rate, see Robert L. Kahn and Charles F. Cannell, *The Dynamics of Interviewing*, New York: Wiley, 1957, pp. 65-91.

¹¹ Charles F. Cannell, "Factors Affecting the Refusal Rate in Interviewing," a paper presented at the 1949 meeting of the American Statistical Association, p. 7 (mimeographed).

Detroit Area Study interviewers were highly trained in that extremely important phase of survey research which occurs when the interviewer first meets the respondent. A discussion of the techniques of probability sampling also is a basic part of interviewer training, for the interviewer often must convince the respondent of the necessity of interviewing him, rather than his wife (who "really knows much more about this than I do") or his neighbor (who "just sits around all day anyway").

In the modern heterogeneous metropolitan community a small number of non-English speaking respondents inevitably are included in samples. The Detroit Area Study was able to handle the majority of such persons by employing a Polish-speaking interviewer, who translated the schedule and interviewed in this language. One per cent of our total sample addresses resulted in an interview rather than a nonresponse because of this provision.

Thirteen per cent of the eligible adults in the sample did not grant an interview. The majority of the nonrespondents, but only eight per cent of the total sample, was classified as refusals.¹² In survey research of this type "refusals" vary from the rare person who is extremely discourteous and seems to gain some psychological satisfaction from slamming the door in an interviewer's face to the timid and retiring housewife who personally would be happy to grant an interview but has been forbidden to do so by her husband.

At only four per cent of the total sample addresses was it impossible to contact the designated respondent.¹³ The incidence of not-at-homes was kept to a minimum by interviewer calls at any hour of the day or night which would suit the convenience of the respondent. Also, interviews were occasionally made at places of work or recreation.

Around one per cent of the adult residents of any large community cannot be interviewed because of physical or mental defi-

ciencies. Short of excluding them from the sample by definition, clearly a questionable procedure, no amount of effort or money can reduce the number of such persons.

THE PROBLEM OF CALL-BACKS

By far the greatest contribution to a successful survey is the willingness to make repeated calls at dwellings which do not yield interviews on the first call. No specific limit to the number of calls per dwelling was established for the Detroit Area Study research. Interviewers continued their efforts until one of four contingencies occurred: (1) an interview was granted, (2) a final and definite refusal resulted, (3) the designated respondent was judged physically or mentally incapable of granting an interview, or (4) a satisfactory response rate was reached.

Call-backs are an expensive element in survey research.¹⁴ Two basic techniques have been developed in an attempt to reduce or eliminate the need for repeated calls with probability samples. The first device involves the selection of a subsample of that portion of the original sample that cannot be reached on the first call. Intensive effort is then made to obtain complete returns from the subsample. This scheme includes appropriate weighting of the original sample and the subsample so as to achieve an unbiased result at lowest cost.¹⁵ In the second technique interviewers call on each sample address only once. This visit is made at a randomly designated time. Each respondent supplies information about his presence or absence from home during a randomly selected time on each day of the week preceding the single interviewer contact. Weights are assigned on the basis of probabilities of being interviewed

¹⁴ J. Durbin, however, claims that while repeated contacts necessarily raise the total cost of a survey, the average cost per interview is not increased greatly by call-backs. "Non-Response and Call-Backs in Surveys," *Bulletin of the International Statistical Institute*, 34 (1954), pp. 3-17.

¹⁵ See Morris H. Hansen and William N. Hurwitz, "The Problem of Non-Response in Sample Surveys," *Journal of the American Statistical Association*, 41 (December, 1946), pp. 517-524. Although this article specifically refers to first contacts based on mail questionnaires, the principles discussed here are applicable to standard personal interview surveys.

¹² This refusal rate is in line with that found in similar surveys reported by Frederick F. Stephan and Philip J. McCarthy, *Sampling Opinions*, New York: Wiley, 1958, p. 262.

¹³ Stephan and McCarthy (*Ibid.*, pp. 241-242) indicate that in most surveys it may be expected that the noncontacted group will run between 3 and 8 per cent of the total sample.

on the first call. The weighted sample is then used in all further analysis.¹⁶

Considerable controversy exists concerning the reliability of each of these methods, their administrative efficiency, the problems they introduce in analysis, the degree to which they require an accurate and difficult to obtain foreknowledge of interviewing costs, and the degree to which over-all costs are actually reduced through their use.¹⁷ Until these techniques are further refined or new methods for eliminating call-backs are devised, most survey researchers would probably prefer to lower the nonresponse rate by repeated, although costly, interviewer contacts.

NUMBER OF CALLS FOR RESPONDENTS AND NONRESPONDENTS

The Detroit area research necessitated an over-all average of almost three individual contacts at each sample address to achieve the desired number of interviews. The mean number of calls, moreover, shows great variation according to the final disposition of the address (see Table 1). It is perhaps paradoxical that completed interviews evidence by far the lowest mean number of calls. The average interview was obtained after only 2.6 calls; interviewers found it necessary to call on the average nonrespondent almost twice as frequently.

Not-at-homes have a substantially higher mean number of calls than refusals. Six or seven contacts were made, on the average, in an effort to reach those adults who are at home only to sleep or who may be on vacation or, possibly, who never answer the door. Our experience with this group of unreachables indicates that for all practical

purposes they are almost impossible to contact under any conditions. Obviously, "increasing the number of calls is not enough, by itself, to do away with [sample] mortality."¹⁸

The average refusal was not so designated until 4.6 calls were made at his home. Only the most adamant nonrespondents were classified as refusals on the basis of a single display of uncooperativeness. In these very rare cases it did not appear physically safe for the interviewer to attempt to complete an interview.

Most adults who refused on the first contact were sent a second letter which again explained the study and pleaded for cooperation. Approximately one-half of these persons later granted an interview when contacted by a different interviewer. The likelihood of a response or a nonresponse resulting from a given call is understandably influenced by the previous calls that may have been made at that address. Well-trained interviewers record all information produced by earlier unproductive calls that may be helpful in winning an eventual interview.

The chances of a successful first call, at least in metropolitan Detroit, are no better than three out of ten (see Table 1),¹⁹ and are about equal to the chances of finding no one at home in the dwelling. In two out of ten first contacts the designated respondent either straightforwardly refuses or states that he is too busy at the moment to grant an interview. When the latter claim is made, the interviewers attempt to obtain an appointment when an interview will be granted.

The correct respondent is not at home in 20 per cent of the first calls, although someone is in the dwelling to provide information upon which intelligent call-backs can be planned. Efficient interviewers can often use this person, who is usually the spouse of the designated respondent, as a liaison between

¹⁶ See Alfred Politz and Willard Simmons, "An Attempt to Get the 'Not-at-Homes' into the Sample Without Call-Backs," *Journal of the American Statistical Association*, 44 (March, 1949), pp. 9-31. More recently, Simmons has modified this technique so that two or more calls are required; weights are applied to completed interviews which are appropriate for the number of calls required to obtain a given interview. See Simmons, "A Plan to Account for 'Not-at-Homes' by Combining Weighting and Call-backs," *The Journal of Marketing*, 19 (July, 1954), pp. 42-53.

¹⁷ Reviews and evaluations of each technique are found in Stephan and McCarthy, *op. cit.*, pp. 255-261; and Moser, *op. cit.*, pp. 135-139.

¹⁸ Stephan and McCarthy, *op. cit.*, p. 239.

¹⁹ Researchers in Elmira, New York, and Madison, Wisconsin, also report a ratio of approximately three interviews for every ten sample addresses contacted for the first time. See Robert Williams, "Probability Sampling in the Field: A Case History," *Public Opinion Quarterly*, 14 (Summer 1950), pp. 316-330; and Lowe and McCormick, *op. cit.* An English study, however, was able to achieve a four in ten ratio for successful first calls; Moser, *op. cit.*, p. 135.

TABLE 2. DISTRIBUTION OF CALLS FOR COMPLETED INTERVIEWS BY SELECTED DEMOGRAPHIC CHARACTERISTICS OF RESPONDENTS

Demographic Characteristics	Number of Cases	Percentage Distribution by Number of Calls					Mean No. of Calls
		One	Two	Three	Four or More	Total	
All Respondents	2,313	32	30	14	24	100	2.6
Employment Status							
Employed	1,357	21	31	18	30	100	3.0
Not employed	952	48	28	9	15	100	2.1
Age							
21-29 years	508	34	28	14	24	100	2.5
30-39 years	634	28	33	14	25	100	2.7
40-49 years	455	29	34	15	22	100	2.6
50-64 years	502	32	25	16	27	100	2.8
65 years or older	212	47	25	11	17	100	2.2
Marital Status							
Never married	186	28	21	14	37	100	3.3
Divorced, separated	105	26	24	16	34	100	3.2
Widowed	163	45	23	11	21	100	2.3
Married, no children present	864	31	26	16	27	100	2.8
Married, children present	990	33	36	13	18	100	2.3
Sex							
Male	1,088	25	27	20	28	100	2.8
Female	1,225	38	30	12	20	100	2.4
Relationship to Head of Household							
Head	1,176	26	29	17	28	100	2.9
Wife	910	40	31	11	18	100	2.3
Other relative	189	33	31	14	22	100	2.6

the study and the desired individual. If the interviewer can impress the value of the research upon the person first contacted, his task in completing the interview will be much simpler. As a result of information acquired from the first call, the likelihood of completing an interview is somewhat higher on the second contact than on the first. The proportions of refusals and respondents not at home decline correspondingly between the first and second calls. Easily contacted respondents, however, are relatively quickly removed from the uncompleted sample; the proportion of responses drops back to the first call level on third and later contacts.²⁰

The number of dwellings at which no one can be found at home becomes a progressively larger segment of the noninterviewed sample with each succeeding call. The discovery of data about these persons which

would aid in making call-backs on a more informed basis is very difficult. Although neighbors are frequently contacted in an effort to ascertain something about their daily routines, these are usually unknown to the neighbors themselves. Thus, fully 43 per cent of the calls made after four contacts are at dwellings in which no one is at home.

The number of calls required to reach a satisfactory response rate with the Detroit Area Study's sample appears to be considerably larger than those reported for other surveys.²¹ As noted above, most of the detailed information now available on this aspect of survey research is derived from studies of non-metropolitan areas. Interviewing mobile metropolitan residents, some of whom live in highly inaccessible apartment buildings, is a much more difficult task than is similar research in smaller communities or rural areas. As the first line of Table 2 indicates, if Detroit Area Study researchers

²⁰ In smaller communities the proportion of responses continues to increase at least through the third call. Williams, *op. cit.*; Lowe and McCormick, *op. cit.*

²¹ Stephan and McCarthy, *op. cit.*, p. 242; Lowe and McCormick, *op. cit.*

had stopped with just two calls, hardly more than six out of every ten eventual respondents would have been reached. Even three calls would have resulted in a response rate of but 66 per cent, and only three-quarters of the final number of completed interviews would have been accumulated.²²

The fact that some respondents can be easily reached with one call while others require eight or nine contacts before granting an interview would not influence a sample based on only one or two contacts if these groups of persons were alike on every characteristic except accessibility. That they are quite dissimilar in many respects is seen in Tables 2, 3, and 4, where selected characteristics of respondents are related to the number of calls necessary to complete an interview.

Demographic Characteristics and Call-Backs. Adults who do not work outside the home are quite easily reached in a fixed-address sample survey. Almost one-half of all unemployed respondents grant interviews on the first contact (see Table 2). Only one-fifth of the employed respondents are interviewed without call-backs.²³ Correspondingly, twice as large a proportion of workers are interviewed after four or more calls (30 per cent) as of nonworkers (15 per cent).

Age bears no constant relationship to accessibility for respondents who are less than 65 years old. Very young adults are about as easily contacted as are middle- or late middle-aged respondents. Beyond age 65, however, the proportion of interviews obtained on the first call rises significantly. The rate of retirement is high in this age group and the general pace of life has slowed. Older respondents, forced to spend many

hours in their homes, are quite easily contacted and no doubt welcome the diversion offered by the interview.²⁴

Unmarried and divorced or separated adults would be significantly under-represented in a study based on even three interview attempts. These persons have little to keep them at home where they can be conveniently reached by interviewers. On the other hand, widowed adults would be weighted too heavily in the limited call-back survey. A very large proportion of all widowed respondents (45 per cent) grant interviews on the first contact. The comparative ease with which the widowed are interviewed is consistent with the tendency for older persons, many of whom are not in the labor force, to be highly accessible.

Whether or not married respondents are easily contacted is related to the presence or absence of children in the house. If there are children the chances are quite small that both spouses are working. Children act as anchors which keep wives, and probably husbands, where the interviewer can reach them relatively quickly. Married respondents with no children are only slightly less difficult to contact than are unmarried adults.

Men, most of whom are in the labor force and unavailable during the day, are much more difficult to contact than are women. Almost one-half of the male respondents in our sample required at least three calls. Only one-third of the females were equally difficult to interview. The implications of this well-documented finding are obvious for limited call-back attitudinal studies in which sex is an important variable.

The relationship of the respondent to the head of the household is also an important factor related to his accessibility. Wives are comparatively easy to reach while the head himself is noticeably more difficult to interview.

Our data do not reveal differences in accessibility associated with race, residence in the suburbs or the central city, or the place of birth of respondents.

Socio-Economic Characteristics and Call-

²² Again, comparison of these figures with comparable data from Elmira and Madison emphasizes the greater effort required when interviewing in metropolitan areas. Williams, *loc. cit.*; Lowe and McCormick, *op. cit.* See also, H. F. Lydall, *British Incomes and Savings*, Oxford: Basil Blackwell, 1955, p. 4; and Cannell, *op. cit.*

²³ All specific statements of percentage point differences and differences in means which are found in the text are statistically significant at the 5 per cent level or better. In computing tests of significance allowance was made for the effects of clustering in the sample. The general conclusions of this paper are also based on statistically significant differences.

²⁴ Stephan and McCarthy (*op. cit.*, pp. 243-244) cite several earlier studies of call-backs in survey research which reach similar conclusions as to the relationship of accessibility to age and to other variables discussed in this section of the paper.

TABLE 3. DISTRIBUTION OF CALLS FOR COMPLETED INTERVIEW BY SELECTED SOCIO-ECONOMIC CHARACTERISTICS OF RESPONDENTS

Socio-Economic Characteristics	Number of Cases	Percentage Distribution by Number of Calls					Mean No. of Calls
		One	Two	Three	Four or More	Total	
Annual Family Income							
Less than \$3,000	264	49	21	10	20	100	2.3
\$3,000-\$4,999	516	34	31	14	21	100	2.5
\$5,000-\$6,999	582	31	31	14	24	100	2.6
\$7,000-\$9,999	505	28	29	14	29	100	2.8
\$10,000 or more	368	25	31	20	24	100	2.8
Median income	...	\$5,500	\$6,250	\$6,600	\$6,450	\$6,100	...
Occupation of Persons Now Working							
Operatives, service workers, laborers	529	24	28	18	30	100	2.9
Craftsmen, foremen	246	21	31	20	28	100	2.9
Clerical, sales workers	291	18	34	17	31	100	3.0
Professionals, managers, proprietors	291	20	32	18	30	100	3.1
Education							
6 years or less	259	33	29	12	26	100	2.7
7-8 years	404	34	28	13	25	100	2.6
9-11 years	580	32	30	15	23	100	2.6
12 years	679	32	29	16	23	100	2.6
Some college	387	30	32	15	23	100	2.6
Mean number of years	...	10.2	10.4	10.6	10.2	10.3	...
Subjective Social Class*							
Working class	431	35	30	15	20	100	2.5
Middle class	20	32	32	18	18	100	2.5

* Data from 1958 survey.

Backs. Socio-economic status is perhaps the primary "independent" variable in most studies of social organization. Definite evidence is shown in Table 3 that the number of calls a researcher is willing to make influences the socio-economic composition of his completed sample.

Family income and the accessibility of family members are inversely related. The chances of obtaining an interview on the first call approach one in two for respondents whose family income is less than 3,000 dollars annually; they drop to one in four if the respondent's family income is 10,000 dollars or more. The annual median family income in greater Detroit between 1955 and 1957 was 6,100 dollars. A median based on first calls only would have resulted in a figure fully 600 dollars below the over-all median.²⁵

²⁵ These findings tend to substantiate Parten's claim of the greater accessibility of lower income respondents. Mildred Parten, *Surveys, Polls, and Samples*, New York: Harper, 1950, pp. 413-414.

The relationship between income and accessibility is probably a result of differential employment rates among families of varying incomes. The more adults in the family who are working, the less likely is anyone to be home at any given time to grant an interview. Also, family income is quite low for older households with no wage earners and increases with the number of workers in a family who contribute to it.

Notwithstanding the relationship between family income and number of calls, additional measures of socio-economic status, such as education, occupation, and subjective social class placement, are not related to accessibility of respondents, at least in the Detroit metropolitan area.²⁶ This lack of relationship reinforces the view that upper-

²⁶ Nor were occupation and accessibility related in Madison. Lowe and McCormick, *loc. cit.* A slight tendency was noted in Elmira, however, for education and accessibility to vary inversely. Philip J. McCarthy, "Sampling Procedure for the 1948 Voting Study," p. 37, (mimeographed).

TABLE 4. MEAN NUMBER OF CALLS FOR COMPLETED INTERVIEWS BY EMPLOYMENT STATUS AND AGE AND SEX, MARITAL STATUS, AND FAMILY INCOME

Age and Sex, Marital Status, and Family Income	Employed		Not Employed	
	No. of Cases	Mean No. of Calls	Mean No. of Calls	No. of Cases
Age and Sex				
21-39 years: men	488	3.0	2.1	38
women	213	3.1	2.3	398
40-59 years: men	354	2.9	1.9	30
women	171	3.0	2.3	256
60 years or older: men	108	3.0	2.4	68
women	22	3.0	1.9	158
Marital Status				
Never married	146	3.5	2.5	37
Divorced, separated	67	3.6	2.2	37
Widowed	55	2.9	2.0	108
Married, no children present	546	3.1	2.3	318
Married, children present	541	2.6	2.0	449
Annual Family Income				
Less than \$3,000	76	3.0	2.0	185
\$3,000-\$4,999	311	2.8	2.0	205
\$5,000-\$6,999	346	2.9	2.2	236
\$7,000 or more	589	3.1	2.1	281

status persons, as such, are not necessarily less accessible than are lower-status adults. The basic consideration in accessibility is whether or not the designated respondent is in the labor force. First calls result in interviews in a disproportionate number of very low-income families because these units are apt to have no working members and are extremely unlikely to have multiple wage earners.²⁷

The degree to which accessibility is a function of employment status is emphasized by the data presented in Table 4. Clearly, when employment status is held constant the relationship of age and sex to accessibility disappears. If older women are working, for example, they are as difficult to interview as are young men. The tendency for adults with children in the home to be more easily contacted than others, although considerably reduced in effect, holds whether or not the parent is working. Moreover, among all employed respondents, those persons who are single, divorced, or separated continue to require more interviewer calls than do married adults or widows. Differences in family income which are associated with accessibility disappear completely when the influ-

ence of employment status on income is removed.

The traditional characteristics upon which most quota samples are based are age and sex. The relationship of these variables to accessibility appears to be primarily a result of differential employment rates. Quota samplers should consider the possibility of adding an additional restriction, employment status, in the design of their research, particularly when they are surveying communities in which many women are employed, or the number of unemployed and retired is large.

DIFFERENTIAL RESPONSE RATES

An over-all response rate of 87 per cent was cited above for the 1956-58 Detroit Area Study surveys. The response rates for specific population types in the sample, however, do not necessarily match those for the total sample, and data for certain segments of the population may be seriously biased by differential response rates. In fact, considerable variation exists among some subgroups in the degree to which our interviewers were successful in their tasks (see Table 5).

Ninety-one per cent of all adults between the ages of 21 and 34 years in the sample granted an interview. Only 78 per

²⁷ "Family Income in Greater Detroit: 1951-1957," *The Detroit Area Study*, 1958 (mimeographed).

TABLE 5. RESPONDENTS AND NONRESPONDENTS BY SELECTED CHARACTERISTICS

Selected Characteristics	Percentage Distribution					No. of Cases
	Re-spondents	Nonrespondents			Total	
		Refusals	Not-at-Home	Others		
Age						
21-34 years	91	4	4	1	100	918
35-59 years	87	8	4	1	100	1,282
60 years or older	78	12	3	7	100	451
Marital Status						
Never married	82	8	9	1	100	226
Divorced, separated	92	3	3	2	100	114
Widowed	74	12	6	8	100	221
Married, no children present	85	9	4	2	100	1,016
Married, children present	93	5	2	*	100	1,069
Sex						
Male	88	7	4	1	100	1,239
Female	87	8	3	2	100	1,412
Relationship to Head of Household						
Head	87	8	4	1	100	1,356
Wife	89	7	3	1	100	1,027
Other relative	86	7	2	5	100	221
Employment Status						
Employed	89	7	4	*	100	1,536
Not employed	86	8	3	3	100	1,111
Race						
White	87	8	3	2	100	2,220
Negro	91	5	4	*	100	428
Total	87	8	4	1	100	2,651

* Less than 0.5 per cent.

cent of the designated respondents 60 years old or older were interviewed. The likelihood of a refusal increases markedly with age, in Detroit as elsewhere.²⁸ But refusals are not the only reason for nonresponse from older persons. Physical and mental incapacities, which occur most frequently among the aged, contribute to a lower response rate in this segment of the population.

The high nonresponse rate among older adults contrasts sharply with the extreme ease with which these persons are contacted on the first call. This apparent contradiction emphasizes the conclusion that accessibility and a high response rate are not necessarily found together. Among all persons who eventually are interviewed, adults over 65 years of age are decidedly more likely to grant an interview on the first call than are respondents of any other age; when all possible respondents are considered, however,

older residents are also much more likely than are younger persons to refuse or to be incapable of granting an interview. The negative influence of age on response rate is evidenced in the low percentage of widowed adults who grant interviews. Only three out of every four widows or widowers in the original sample were interviewed. Both refusals and illness played important roles in the large number of nonresponses shown by this group.

Single adults also tend to have rather low response rates. The nonresponse of unmarried Detroiters is not solely a result of refusals; one-tenth of the single adults in our sample were never contacted by an interviewer. Whether or not many of the designated respondents who are impossible to find at home are simply refusing by avoiding the interviewer is unknown.

Childless married adults have a somewhat lower response rate than do married adults with children. A disproportionate number of designated respondents with no minor

²⁸ Lowe and McCormick, *op. cit.*; Stephan and McCarthy, *op. cit.*, p. 262.

children in the home refuse to be interviewed. These persons are older, on the average, than adults with children, a fact which may explain at least some refusals in this group. The data indicate, however, that even when age differences are controlled, childless adults are slightly less likely to grant an interview than are married persons living with their children. Perhaps those parents who know that they must be home with the children whether or not they are interviewed, are therefore more willing than otherwise to sacrifice an hour of their time in the interests of social research.

Sex, relationship to head of household, employment status, and race are not highly associated with differential response rates. Notwithstanding the relative inaccessibility of men, household heads, and workers, these persons do not contribute higher than average nonresponse rates. Although gaining a completed interview with these respondents requires several call-backs, this fact does not mean that they are likely to refuse or to remain almost permanently inaccessible.

The income, occupational status, and educational levels of nonrespondents were not available. But an indication of the socioeconomic status of nonrespondents who reside in Detroit is provided through the use of data from the 1950 Census. Analysis of rental distributions for the 1955 Detroit Area Study's survey reveals a slight underrepresentation of middle level rental units in the sample.²⁹ In general, however, the median rental of the blocks in which nonrespondents are found is quite comparable to that of the city population as a whole and quite within allowable sampling error limits. The 1950 median rental of 1955 nonrespondents was 46 dollars; the comparable figure for the total city was 44 dollars. These data do not substantiate the rather common claim that upper socio-economic groups contribute a disproportionately large number of final nonrespondents.³⁰

Little evidence exists in the data to support the proposition that refusals and not-at-homes tend to cancel one another.³¹ Although

refusals and age vary together, the proportion of not-at-homes is not related to age differences. Widowed adults have a high refusal rate and at least an average proportion of not-at-homes. Also, while unmarried Detroiters are very difficult to contact, almost as much nonresponse in this group is due to refusals as to not-at-homes.

SAMPLE BIAS DUE TO NONRESPONSE

Although nonresponse rates for a few segments of the population are rather high, this variation does not necessarily imply that strong biases exist in carefully planned probability samples. Table 6 presents some support for the claim that relatively small biases occur in a probability sample of the type described here when the over-all response rate is reasonably high.

Seven characteristics of actual respondents and of all possible respondents in the original sample are compared in Table 6. Twenty-one specific comparisons are made and in only one instance does the bias in the completed sample, which derives from a failure to obtain a 100 per cent response rate, exceed two per cent.³² The sample overestimates the proportion of husband-wife-children family units by 2.5 percentage points. This and all other biases shown in Table 6 are in the directions which would be expected from earlier discussions.

There is no claim here that possible sample bias should be ignored. The objectives of a given research may conceivably dictate that a two percentage point error is unacceptable. In such instances the researcher may choose to incorporate the bias due to nonresponses into the total error of the survey estimates, using methods detailed in the literature.³³ Or, he may choose to handle

Stephan and McCarthy, *op. cit.*, pp. 266-269; Helen Crossley and Raymond Fink, "Response and Non-Response in a Probability Sample," *International Journal of Opinion and Attitude Research*, 5 (Spring, 1951), pp. 1-20.

³² Comparatively small biases are also reported for the Elmira study. Stephan and McCarthy, *op. cit.*, pp. 266-269. Examples of surveys where large nonresponse rates resulted in dangerously large biases are found in Parten, *op. cit.*, pp. 404-417.

³³ See, e.g., Z. W. Birnbaum and Monroe G. Sirken, "On the Total Error Due to Noninterview and to Random Sampling," *International Journal of Opinion Attitude Research*, 4 (Summer, 1950), pp.

²⁹ John Takeshita, "The Interview Response Rate for the 1954-55 Detroit Area Study," *The Detroit Area Study*, 1955 (mimeographed).

³⁰ See, e.g., Parten, *op. cit.*, pp. 412-413.

³¹ Berelson, Lazarsfeld, and McPhee, *loc. cit.*;

TABLE 6. RESPONDENTS AND TOTAL SAMPLE BY SELECTED CHARACTERISTICS

Selected Characteristics	Percentage Distribution		Percentage Point Difference
	Respondents	Total Sample	
Employment Status			
Employed	58.8	58.0	+0.8
Not employed	41.2	42.0	-0.8
Total	100.0	100.0	—
Age			
21-34 years	36.4	34.6	+1.8
35-59 years	48.1	48.4	-0.3
60 years or older	15.5	17.0	-1.5
Total	100.0	100.0	—
Marital Status			
Never married	8.1	8.5	-0.4
Divorced, separated	4.5	4.3	+0.2
Widowed	7.1	8.4	-1.3
Married, no children present	37.4	38.4	-1.0
Married, children present	42.9	40.4	+2.5
Total	100.0	100.0	—
Type of Dwelling Unit			
Apartment house or hotel	9.3	10.0	-0.7
Other multiple dwelling	25.1	25.0	+0.1
Single family dwelling	65.6	65.0	+0.6
Total	100.0	100.0	—
Sex			
Male	47.0	46.7	+0.3
Female	53.0	53.3	-0.3
Total	100.0	100.0	—
Relationship to Head of Household			
Head	50.9	51.2	-0.3
Wife	39.3	38.7	+0.6
Other relative	8.2	8.3	-0.1
Nonrelative	1.6	1.8	-0.2
Total	100.0	100.0	—
Race			
White	83.0	83.8	-0.8
Negro	17.0	16.2	+0.8
Total	100.0	100.0	—
Number of cases	2,313	2,651	—

this problem by adjusting his completed sample for nonresponse bias.³⁴ Nonetheless, for most surveys, biases of the magnitude reported here are quite reasonable and would generally have little influence on the final product of the analysis.

CONCLUSION

The ability of the student of human populations to draw a sample which is repre-

sentative within known confidence limits of a given universe is well established. Even perfectly selected samples, however, can result in very unrepresentative units of observation if the designated sample varies substantially from the final product. The problems of varying accessibility among urban residents, differential response rates, and sample bias due to nonresponse have been discussed in some detail. The experiences reported here, we hope, will benefit future investigators who plan to employ survey techniques in the study of human populations.

179-191; William G. Cochran, *Sampling Techniques*, New York: Wiley, 1953, pp. 292-298.

³⁴ For clear descriptions and defenses of this procedure, see Moser, *op. cit.*, pp. 139-143; and Hansen, Hurwitz, and Madow, *op. cit.*, pp. 56-59.

PREDICTING A COMMUNITY DECISION: A TEST OF THE MILLER-FORM THEORY

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Two proposed constitutional amendments appearing on the November 4, 1958 ballot became issues of general community concern in Denver, Colorado. One, a right to work amendment proposal, aroused a high level of community interest and activity; the other, a civil service amendment proposal, stimulated relatively less community interest and activity. This setting provided an opportunity for an independent prediction test of the Miller-Form theory of issue outcome in community decision making. This theory is discussed briefly, operational procedures for determining the values of the factors of the theory for predictive purposes are described, and results interpreted as a favorable test of the theory are presented. Certain questions concerning reliability, alternative application models, and further research problems are also discussed.

A THEORY of issue outcome in community decision making formulated by Delbert C. Miller and William H. Form was subjected to an initial test in the November election of 1956, when Miller correctly predicted the outcome of voting for and against a "right to work" initiative in Seattle, Washington.¹ The appearance on the State ballot of a right to work amendment proposal in the November 4, 1958 elections in Colorado provided an excellent opportunity for an independent prediction test of the Miller-Form theory in Denver, where this issue had aroused a high level of interest, publicity, and pre-election activity.

Miller claimed only that the theory of issue outcome would predict passage or defeat of an issue. The model used cannot supplant public opinion polling in arriving at accurate estimates of voting percentages in public elections. In the Seattle case, however, Miller's pre-election analysis yielded a prediction of 31 per cent for and 69 per cent against the right to work initiative. The actual returns were 33 per cent for and 67 per cent against. Such a favorable outcome of the test suggests at least the possibility that, for certain purposes and under certain

conditions, the Miller-Form model may yield sufficiently accurate quantitative as well as qualitative predictions to justify its use rather than the far more expensive and time consuming public opinion polling procedures. For example, it may be useful for predicting outcome when settlement of the issue involves no public vote, or when very accurate percentage figures are not the most important goal of the study, or when the researcher cannot conduct a public opinion poll because of lack of resources. Yet, as Miller stresses, the major significance of the model rests upon its ability to provide a test of the researcher's knowledge and understanding of the community power structure and the decision making processes within the community.

THE MILLER-FORM THEORY OF ISSUE OUTCOME

The factors. The theory is based on the proposition that the combined social force of three factors brings about a community decision on a general issue. These factors are: (1) the *critically activated parts of the institutional power structure*, which are aligned for or against the issue proposition; (2) the *power arrangement of the community power complex*, which may be unstructured, semi-structured, or unified on either side of the issue; and (3) the *top influential solidarity and activity*.² If the top influen-

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¹ Delbert C. Miller, "The Prediction of Issue Outcome in Community Decision Making," *Proceedings of the Pacific Sociological Society* ("Research Studies of the State College of Washington," 25 [June, 1957]), pp. 137-147. All quotations are from this article.

² Although Miller includes only "solidarity of the top influentials" as the third factor in his initial presentation of the theory, it becomes evident in the "application" sections of the paper that the degree of activity of top influentials on either side is also a critical variable.

tials are a single, exclusive elite of autonomous power,³ the outcome may be forecast by an analysis of their stand on the issue. If the top influentials are not the exclusive elite type, their degree of activity on either side of the issue becomes a relevant variable.

In the model for the application of the theory, values are assigned to the forces acting on the issue. The sums of these weights result in a ratio of contending forces and a basis for predicting outcome as passage or defeat of the issue proposal.

Definitions of the factors and difficulties in application. Although Miller defines the factors relevant to the theory, the operations necessary to determine the particular factor values for weighting purposes are never made explicit.⁴ For example, the "critically activated parts of the institutional power structure" is defined as follows:

[These elements of the] power structure refer to the institutional sectors whose interests are affected and become alerted in whole or in part. Such sectors may include: Business, Labor, Education, Religion, Political Parties, Government, Society, Independent Professionals, Mass Communication, Recreational, Welfare, and Cultural Institutions. Critical activation occurs in those parts of the institutional power structure which respond with high intensity to an organizational value affected by the issue. Critical activation is identified by analyzing the substance of the issue and the level of the issue. The substance of the issue refers to the content of the issue and the implications for the various institutional sectors. The level of the community issue refers to the "value intensity" exhibited by the affected parties as they appraise the possible impact of the issue outcomes upon their interests.

A number of serious questions concerning reliability confronts the researcher who tries

³ The possible types of top influential groupings range from a pure "democratic" type, through "fluid" and "core" influentials types, to the exclusive elite type.

⁴ The "procedure" information (other than the weighting procedure to follow after the factor values have been determined) consists of this brief statement: "Data about the influential forces operating in the community were secured by interviewing the leaders of the major organizations campaigning on both sides of the issue. Certain other informants were also interviewed. Newspapers were carefully followed over a six-month period." In addition, Miller includes an illustrative account of his designation of the factor values.

to designate the "critically activated parts of the institutional power structure," as defined above. For example, in his final weighting of the "critically activated parts" in the right to work issue, Miller included religion as an activated sector, but not mass communications; he included the Democratic Party, but not the Republican Party.⁵ There seems to be no purely logical reason for these selections. Nor does Miller describe procedures for determining the value intensity level of the issue for the various sectors.

Other questions arise. If *parts* of sectors are allowed activation designation (for example, the Catholic Church in the religious sector), there appears to be no ultimate possible limit to the weighting of the critical activation factor, which would seriously affect the relative weight of the other factors. On the other hand, if only the sectors as wholes are allowed activation, then is it not possible for a sector to be "split," although intensively activated? ⁶ Still unsolved are the problems of adequate definitions of the scope of each sector, the development of a standard list of sectors which would be appropriate for any community, and whether to use equal or differential weights for the sectors in a given community.

Similar problems arise with regard to the other factors. For example, in his directions for applying weights for the "power arrangement of the community power complex," Miller writes: "locate most influential organizations and arrange them according to the mobilized relations. a. To locate most influential organizations, examine resources, leadership, voting strength, and rank of institutional sector in community power profile. b. Determine power arrangement type of community power complex . . ." (that is, whether unstructured, semi-structured, or unified coalition on both sides). Again, procedures for determining the resources, voting strength, rank, and the like of the sectors and directions for deciding how these vari-

⁵ In Denver, both political parties were "activated" under a more stringent operational definition of critical activation; mass communication, society, and welfare, as well as religion, were activated under a more liberal operational definition.

⁶ For example, a split occurred in Denver on the civil service amendment proposal in the critically activated government sector.

ables should be assessed and related to a particular type of power arrangement for weighting are not specified.

A SPECIFICATION OF THE FACTORS IN THE MILLER-FORM THEORY

Although Miller provides nominal definitions of the relevant theoretical factors and describes the weighting system to be used when factor values have been determined, he does not specify the procedures to be used in obtaining the data necessary to yield particular values for the factors in the theory. The problem we faced, then, was how to operationalize the concepts in the Miller-Form theory. The solution adopted was to construct a structured interview schedule to be administered to strategically located persons in the subject community who would presumably know the issue situation in their sector and in the community in general. The consensus of their answers to specific questions would provide the basis for the assignment of values to the factors of the theory. The result would be a replicable method for making factor value decisions in using the theory to predict issue outcome.

The twelve sectors named by Miller in his definition of critically activated parts of the institutional power structure were accepted as those which, on one issue or another, may become critically activated. Three presumably knowledgeable persons from each sector (three representing business, three in the field of labor, and so on—a total of 36) were interviewed during the week prior to the election.⁷

The critically activated parts of the institutional power structure. Each respondent was handed a sheet which named and indicated briefly the scope of each of the sectors. (The "political parties" sector was divided into two parts—Republican and Democrat.) Also on the page were the two questions designed to determine, first, the level of in-

tensity of the issue for each sector—the degree of importance of the issue for a sector; and, second, if "important" or "very important," the stand of the sector—for, against, or split on the issue. As they appeared on the page the items were as follows:

Degree of importance for sector interests:

- a. not important—that is, not important enough for their interests to engage in pre-election activity.
- b. important—that is, important enough to engage in at least some pre-election activity.
- c. very important—that is, so important that intensive pre-election activity is necessary.

If important or very important, the stand of the groups involved:

- a. practically all are for the amendment.
- b. a majority are for the amendment.
- c. they are split—about half for and half against.
- d. a majority are against the amendment.
- e. practically all are against the amendment.

Each respondent was asked to estimate the degree of importance of the issue for each sector, and to answer the follow-up question if warranted by the previous response.⁸ A summary of the responses of the 36 informants to the degree of importance question on the right to work amendment is presented in Table 1. The outcome of the "stand" questions for each activated sector is summarized in Table 4.

The power arrangement type. The "power arrangement of the community power complex" on either side of the issue was determined by the consensus of responses to the following questions:

Now we are interested in your estimate of the degree of cooperation among those groups and individuals who are working for or against the amendment.

Thinking only of the groups which are *against* the right to work amendment, which of the following arrangements best describes the situation as you see it?

- a. The groups which are against the right to work amendment are going it alone, that is to say, there is little cooperation among them.
- b. Some of the important groups are cooper-

⁷ Lists of names of "knowledgeable" persons were secured during initial guided interviews with five long-time residents of the area. The potential respondents were selected from these lists. The suggested names included persons holding positions in formal organizations in the community, officers and executive secretaries of major voluntary associations, various council and board members from the various sectors, etc.

⁸ A "don't know" response to the initial question was treated as equivalent to a "not important" response, i.e., the second question was not asked.

TABLE 1. SUMMARY OF RESPONSES TO QUESTIONS DETERMINING CRITICAL ACTIVATION OF INSTITUTIONAL SECTORS: RIGHT TO WORK ISSUE

Institutional Sector	Responses			
	Not Important	Important	Very Important	Don't Know
Business	1	15	20	0
Labor	0	1	35	0
Mass Communications	2	17	16	1
Religion	17	14	4	1
Welfare	15	13	4	4
Society	11	18	5	2
Independent Professionals	15	20	1	0
Education	13	18	4	1
Recreation	26	6	1	3
Cultural Groups	25	10	0	1
Republican Party	1	15	19	1
Democratic Party	0	9	27	0
Government	5	16	13	2

N = 36

ating, but others have remained independent.

- c. Practically all of the important groups and individuals are united and share resources and leadership in their efforts to defeat the amendment proposal.

What about the different groups of people who are *for* the right to work amendment? Are they working independently, or is there some cooperation, or are they united in their activities for the passage of the amendment?

A summary of the responses to these questions is shown in Table 2. Note that the modal categories indicate a united effort against the amendment, but only a semi-structured arrangement for the right to work amendment.

The solidarity and activity of top influentials. Solidarity among top influentials was

TABLE 2. SUMMARY OF RESPONSES TO QUESTIONS DETERMINING POWER ARRANGEMENT OF THE COMMUNITY POWER COMPLEX: RIGHT TO WORK ISSUE

Power Arrangement Type	For the Amendment	Against the Amendment
Independent (unstructured)	5	0
Some Cooperation (semi-structured)	18	5
United (unified coalition)	13	30
Don't Know	0	1
Total	36	36

determined by the modal response to the following question:

Now we would like you to consider only the top 40 or 50 community leaders in Denver. Which of these four alternatives is the case?

- All of the top leaders here are for the right to work amendment. (Ask follow-up A only.)
- All of the top leaders here are against the right to work amendment. (Ask follow-up B only.)
- There is a split among top leaders on this issue. (Ask both A and B.)
- None of the top leaders have taken a stand on this issue. (Skip both A and B.)

Depending on the response to this question, two follow-up questions were asked to yield estimates of top influential *activity* for and against the amendment proposal:

- A. Among the top leaders who are *for* the right to work amendment, which best describes their degree of supporting activity for the amendment?

- Leaders who are for the right to work amendment are *not* supporting it at all.
- They are supporting it with contributions, but most of them are not making speeches or publicly demonstrating support.
- As a whole, they are supporting it very actively with contributions, speeches, and public appearances.

- B. Among the top leaders who are *against* the right to work amendment, would you say that they:

- are not fighting it at all.
- are only giving contributions, but are not publicly active.

TABLE 3. SUMMARY OF RESPONSES TO QUESTIONS DETERMINING THE SOLIDARITY AND ACTIVITY OF TOP INFLUENTIALS: RIGHT TO WORK ISSUE

Solidarity	Responses	Activity	For	Against
All for	0	Inactive	1	3
All against	1	Supportive	27	4
Split	33	Active	3	27
Uncommitted	2	Don't know *	5	2
	—		—	—
Total	36		36	36

* Or not relevant because of answer to the solidarity question.

3. are very active with contributions, speeches, and public appearances.

The summary of responses to the questions concerning the solidarity and activity of top influentials is presented in Table 3. Note again the consensus of respondents on the greater activity of top leaders against the right to work amendment.

Weighting and analysis of the data, and the prediction. The following weighting system, described and employed by Miller, was used: (1) Each critically activated sector received a value of 1, for or against (or 0 if split, because of cancellation of effect). These weights represent the independent influence of each activated part. (2) Multiplier weights of 1 for unstructured, 2 for semi-structured, and 3 for the unified coalition power arrangement type, on each side of the issue, were assigned, and these weights were used to multiply the pro and con totals of the activated sectors. The resulting values represent "the interactive, reinforcing influence of organizations in extending the range of attitude formation and increasing intensity of opinion brought to bear upon the issue." (3) The influence of top influential activity—2 for active, 1 for supportive, 0 for inactive—was added. (4) The final total on each side of the issue—the sum of the critically activated parts, plus the result of the power arrangement type multiplication, plus the top influential activity value—represents the summation of influential forces affecting issue outcome. The outcome was predicted according to the ratio of contending forces.

In our analysis of the data to determine critical activation of sectors, two alternative cutting points were used. Under the more stringent interpretation of activation (Plan I in Table 4), eighteen or more of the respondents must have termed the issue "very

important" for the interests of the sector.⁹ The stand of the sector for or against the issue was determined by inspection of the predominance of responses (collapsing a and b *versus* d and e responses) in the stand question. At least twelve of the 36 respondents must have judged a sector "split" or a clearly bi-modal distribution must have been observed before the split designation of a sector's stand was accepted.¹⁰ Under the more liberal interpretation of critical activation (Plan II in Table 4), a sector was accepted as activated *unless* eighteen or more respondents judged the issue "not important" for a sector's interests.

The designation of the values for type of power arrangement and for top influential activity factors was determined by the modal category of responses on the questions inquiring about these variables.

These alternative procedures produced two final ratios, both predicting defeat of the right to work amendment proposal. The more stringent Plan I yielded a predictive percentage of 59 per cent against the proposal; the more liberal Plan II percentage was 69 per cent against. The actual percentage of the vote against the right to work amendment proposal in Denver was 65.8 per cent, a figure bracketed in by our estimates.¹¹

⁹ The probability of 18 of 36 responses falling in one of three response categories by chance alone is less than 5 in 100.

¹⁰ The probability of 12 of 36 responses occurring by chance alone in one of 5 response categories is less than 5 in 100.

¹¹ It is interesting, though not quite valid, to compare these results with those obtained in a "poll among a scientific cross-section of voters in all parts of the state," conducted by Research Services, Inc. for *The Denver Post*. The following results were reported in the October 15 issue of the paper on the "right to work" proposal: Against—38 per cent; For—35 per cent; Undecided—27

TABLE 4. SUMMARY TABLE DEMONSTRATING SECTOR CRITICAL ACTIVATION UNDER TWO ANALYSIS PLANS,* WEIGHTING PROCEDURES, AND PREDICTION RATIOS COMPARED WITH ACTUAL OUTCOME

Community Sectors	Weights							
	Not Activated		Split		Activated For		Activated Against	
	I	II	I	II	I	II	I	II
Business					1	1		
Labor							1	1
Mass Communications	0							1
Religion	0							1
Welfare	0							1
Society	0					1		
Independent Professions	0			X				
Education	0			X				
Recreation	0	0						
Cultural Groups	0	0						
Political Parties								
Republican					1	1		
Democrat							1	1
Government	0			X				
Totals:					2	3	2	5
Power Arrangement Type					For		Against	
Unstructured (1)								
Semi-structured (2)					2	2		
Unified coalition (3)							3	3
Summary (power arrangement type X activated sectors)					4	6	6	15
Top Influential Activity								
Inactive (0)								
Supportive (1)					1	1		
Active (2)							2	2
Final Summary (critically activated sectors Totals plus power arrangement type Summary plus top influential activity)					7	10	10	22
Ratios and Predictive Percentages					Actual Outcome			
Plan I: 7:10 for, or 41 per cent for, 59 per cent against					For: 56,115 or 34.2 per cent			
Plan II: 10:22 for, or 31 per cent for, 69 per cent against					Against: 108,259 or 65.8 per cent			

* Plan I: critical activation—18 or more "very important" responses.

Plan II: critical activation—unless 18 or more "not important" responses.

A summary of these procedures, the predictions, and the actual outcome on the issue is presented in Table 4.

per cent. The report noted that the against vote was higher in Denver, but no figures were cited. The right to work proposal failed in the state as a whole by a 3 to 2 ratio, roughly 60 per cent against to 40 per cent for. A previous poll, reported in the September 12 issue of the *Denver Post*, disclosed that 51 per cent of voters questioned in "Colorado cities" would vote against right to work. Figures for the Denver area were reported as 55 per cent against, 28 per cent for, and 17 per cent undecided. In the same article, results of an early March poll were mentioned which indicated confusion among voters over "right to work" and "union shop." Voters then were in favor of both, with 56 per cent of Denver voters saying they favored "right to work," evi-

THE CIVIL SERVICE AMENDMENT PROPOSAL

Miller proposed that the predictive model would be valid when the community was highly activated on "an issue of great concern." In order to try to gain some knowledge about the validity of the model over a wider range of issues, an issue of less community concern was included in the test. The issue selected was a civil service amendment proposal which had aroused much less general interest and pre-election activity in Denver than the right to work proposal.¹²

dently not understanding that the "right to work" amendment would prohibit union shops.

¹² The October 15 poll cited in footnote 11 in-

Using the procedures described above, the following results were obtained. Under Plan I, only Government was critically activated, but it was "split" leaving zeros as totals on the critically activated parts factor. Although the power arrangement type values differed (semi-structured *for*, unstructured *against*), the multiplication technique resulted in zero summaries. Finally, the modal response on the top influential activity question was "inactive," or zeros on both sides of the issue. Consequently, the ratio of contending forces amounted to 0:0, or 50/50. Under Plan II, no sectors were activated against. Welfare, Republican Party, and Government were split. Four sectors were critically activated for: Labor, Mass Communications, Education, and Democratic Party. Multiplying 4 by the semi-structured power arrangement type weight 2 yielded 8 for to 0 against. Hence our prediction was that the civil service amendment would pass but the percentage prediction range was as wide as possible—from 50 to 100 per cent.

Although the civil service amendment proposal failed to pass in the state as a whole, the actual outcome in Denver was 54 per cent for and 46 per cent against. Again our prediction bracketed in the result. The wide range of the prediction estimate may be interpreted as a consequence of this procedure when the issue arouses relatively little concern. When our confidence in the data decreases it is appropriate that the predictive range of estimates increases. The greater number of "don't know" responses on the civil service questions also supports this interpretation.¹³

cluded the question: "Which of the five amendments to the Colorado constitution to be voted on in November are you familiar with now?" Of those questioned, 56 per cent were familiar with the right to work amendment and only 23 per cent with the civil service amendment over the state as a whole. On the civil service amendment, voters at this time were reported to be 34 per cent for, 10 per cent against, and 56 per cent undecided. No figures were given for Denver. The poll failed to predict direction since the civil service amendment was rejected in the state as a whole by a slight margin, about 52 per cent against and 48 per cent for.

¹³ On the right to work proposal, there were 16 "don't knows" on the degree of importance question (see Table 1, column 4); on the civil service proposal, there were 24 "don't knows" on the same question.

EX POST FACTO ANALYSES OF THE DATA

Other questions concerning reliability and the possible usefulness of the prediction model can be probed by statistical tests.

Respondent agreement. One question that can be tentatively answered is the extent to which there is agreement among the twelve groups of three respondents on questions about the degree of importance for both the right to work and civil service issues. Using the Friedman two-way analysis of variance test,¹⁴ we accepted the null hypotheses in both cases that the sector respondents were drawn from the same population. But we rejected the null hypotheses in both cases that the questions were drawn from the same population, which may be interpreted positively as meaning that the respondents from different sectors agreed in their differentiation of the "importance" level for the various sectors on both the right to work and civil service issues (at the .001 level of confidence).

Interviewer reliability. In this study, nine different interviewers each interviewed four people representing four different sectors.¹⁵ Therefore it is appropriate to ask whether the respondents of one interviewer were in general agreement or disagreement with the others. An unfavorable result on this test would indicate interviewer bias, and cast doubt on the reliability of our procedures. Again using the Friedman test, however, we found no apparent interviewer effect: the null hypotheses that the interviewers were drawn from the same population were accepted; rejecting the null hypotheses at the .001 level that the questions were drawn from the same population is to say positively that the four respondents of the different interviewers agreed in their differentiation of importance levels for the various sectors on

¹⁴ Weights were assigned for ranking purposes in the Friedman test as follows: Not Important and Don't Know—0; Important—1; Very Important—2. Responses of the three respondents were summed, and ranks determined from these raw weights, following the test procedure. See Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences*, New York: McGraw-Hill, 1956, pp. 166-172.

¹⁵ With one exception: one interviewed five and one three persons. To facilitate rank ordering in the Friedman test, the "exception" case was treated as belonging to the person with only three interviews.

both the right to work and civil service amendment questions.

Sector respondent variation. When accurate information about a community situation is desired from a group of respondents, as in the case of the use of our prediction model, it is conceivable that respondents from one sector consistently estimate the situation more accurately than respondents from other sectors. Assuming that the group mode is the best estimate of the situation, the "best informants" question was explored by means of the following procedure.

A rough measure of the variation of each sector from the total group mode was devised. On the level of "importance" questions, weights were assigned to the possible response categories, as in the two tests described above: Not Important or Don't Know—0; Important—1; Very Important—2. The three respondents' scores in each sector were summed for each question. This sum was then subtracted from a "perfect" score representing the score obtained from tripling the total group mode score. For example, the group modal response was "very important" (or 2) for the business sector on the right to work amendment proposal; the perfect mode score then is 3×2 or 6. Three respondents from a sector who made this same judgment would score $2 + 2 + 2$, or 6. If one respondent had indicated "important" rather than "very important," that sectors' score would be $2 + 2 + 1$, or 5. The deviations of the sector groups' scores from the perfect mode scores were squared and summed for each question. Expressed in a formula, the variation score of each sector = Σ (perfect mode score — sector group score)². The variation sum for each sector represents roughly its degree of disagreement with the modal response of the total group.

The various sector groups can be ranked in the order of "least variation" (or most agreement with the modal responses) on the level of importance questions. For the right to work amendment the rank order is: 1. Education. 2. Business. 3. Cultural Groups. 4. Independent Professions. 5. and 6. Welfare and Government. 7. Mass Communications. 8. Political Parties. 9. Society. 10. Labor. 11. Religion. 12. Recreation. On the civil service amendment questions, the same groups of respondents from the different sec-

tors, in order from most to least agreement with modal responses, rank: 1. Mass Communications. 2. Society. 3. Independent Professions. 4. Political Parties. 5. and 6. Labor and Education. 7. Cultural Groups. 8. Religion. 9. Welfare. 10. Recreation. 11. Government. 12. Business.

The Kendall Rank Correlation Coefficient Tau between the two orders is $-.16$. This result suggests that a particular group of respondents from a given sector does not do equally well in judging the community situation on all issues. On one issue, respondents from a sector consistently judge importance levels about the same as does the group as a whole; on another, the same respondents may often deviate considerably from the group's modal responses.

Alternative weighting systems. Following the election, members of the investigating group were asked to devise alternative data analysis schemes and weighting procedures to see if they could obtain results from the respondent data closer to the election returns than our pre-election prediction based on the Miller weighting system. Recognizing that the number of possible models for applying the Miller-Form theory is very large and that exploration of the data in this way is frankly manipulative and *ex post facto*, our objective was to devise and evaluate various models for possible application in a predictive test situation on another occasion.

The models suggested were all interesting and, of course, with regard to the right to work issue, could be used to "predict" the actual outcome to within one or two per cent. These different applications cannot be discussed here, but it may be worthwhile to mention the essential features of two models which made use of all the data collected and which "predicted" both the right to work and the civil service actual votes to within one per cent. These two plans incorporated into the theoretical base of the models the additional assumption that a conservative force for the status quo gives the "against change" side an advantage that should be taken into account in the weighting system leading to a forecast of the actual voting outcome. One plan is essentially a variation of the Miller weighting procedure, except that non-activated sectors are given some weight as a conservative force, and the values

of the other factors are increased to allow for the larger total value of the critically activated parts factor. The other plan might be called a "force vector model," which converts individual respondent data into additive standardized weights for (Vector X_a) and against (Vector X_b) the issue proposal. The final "for" proportion prediction formula, which makes the vote proportional to the vector forces, includes a constant "conservatism" factor (.85), as in the formula:

Vote For = .85 ($\frac{X_a}{X_a + X_b}$). Both of these models, we believe, deserve predictive testing on other occasions.

Finally, it has been suggested,¹⁶ as in our Plan I and Plan II prediction percentage range, that application models should include allowance for the confidence with which the researcher makes his prediction. In other words, confidence intervals, with a lower and upper limit for percentage predictions, may be narrow in range for some predictions and wide in range for others, depending on the researcher's measure of confidence in his data. One possibility would be to use the proportion of "don't know" responses as a rough guide for confidence in the data, extending the interval as the proportion of "don't know" responses increases. Another confidence function might be the size of the sample of respondents.¹⁷

FURTHER RESEARCH PROBLEMS

A mark for the validity of the Miller-Form theory of issue outcome is the fact that it has been successfully tested independently under different operational procedures. But both tests occurred in large cities and state capitals, and in both cases the outcome on the same kind of issue (the right to work amendment proposal) was decided on the basis of a public election. Evidence from the Denver study on the civil service amendment proposal indicates that less confidence should be placed in a prediction obtained from an

application model of the theory when the issue does not arouse a high level of community interest and activity.

Perhaps the major practical research problem is to establish a generally applicable and replicable method for determining sector activation and weighting. Sector definitions should be refined and standardized for different sizes and types of communities, and a theoretically based rationale for whatever weighting system is adopted should be formulated. A procedure for establishing the relative power rank of the sectors, for example, might be developed which could be applied when values are assigned to activated sectors.

Different kinds of situations concerning community issues may require different types of application models. For example, predictive tests of issue outcome might be attempted for cases in which public elections do not determine the outcomes. The problem of establishing the minimum number of respondents needed to secure adequate and reliable information under different situations also calls for exploration. Finally, whether the model is sensitive enough to detect changes in sentiment as an issue approaches its climax is not known at present. As in political polling, a prediction based on the model must assume no significant changes in force alignments between the time of the prediction and the actual outcome.

While it is obvious that application models of the Miller-Form theory rest upon assumptions about the adequacy of information from a small number of respondents, and also upon arbitrary weighting procedures, it is equally apparent that if prediction models consistently "work" they must be tapping real community forces affecting community decisions.

It has been noted that the theory, in effect, ignores the fact or probability of interdependence among the sectors it analyzes as independent "parts" for the purpose of making a prediction. But there is no reason why sector interdependencies cannot be investigated with the present model. It is conceivable that in a given community, certain parts *always* become activated and stand together in the same way, regardless of the issue. Perhaps more probable is a regularity of pattern among certain sectors which vary with the type of issue. On economic issues,

¹⁶ By Edward Rose of the University of Colorado.

¹⁷ In the Boulder pre-test of the schedule, only nine persons were interviewed. Analysis of these data yielded correct predictions of the direction of the voting, but the quantitative predictions overestimated the percentage vote for the defeat of the right to work proposal and the passage of the civil service amendment proposal in Boulder.

for example, one pattern of relationship among sectors may persist, while another may show itself on educational issues. Postulated sector relationship patterns of this kind would likely vary with different types of communities. Clearly, it would require an intensive program of longitudinal and com-

parative community research to establish such regularities. Moreover, to account for variations in regularities observed in different types of communities demands a more powerful theory of community structure and community decision making than we have available at present.

EQUILIBRIUM AND THE PROCESSES OF DEVIANCE AND CONTROL *

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How appropriate is the equilibrium notion for the analysis of deviance and control processes in the personality and in the small social system? Issues arising in the application of a selected notion are discussed. It is suggested that the model is genuinely useful only when the concrete system in question corresponds to a hypothetical set of conditions called the simple system. Because of certain inherent properties of behavioral systems, such a correspondence seems, at best, infrequent. For these rare instances, the question of whether sanctions tend to have an equilibrating effect seems a relevant one. For other instances, however, the elementary issue is whether the system as a whole approaches or departs from the simple system.

THIS paper explores certain theoretical issues that arise in applying an equilibrium model to processes of deviance and control in human behavioral systems. A number of social scientists have employed one or another of the notions of equilibrium in order to comprehend complex processes in such systems, be they personalities, as in the cases of Lewin¹ and Parsons and Shils,² or social systems, as in the cases of Pareto,³ Chapple and Coon,⁴ Homans,⁵ and Parsons, Shils, and Bales.⁶ With respect to its popu-

larity, Easton recently went so far as to say that "... it represents perhaps one of the few analytical orientations common to all social research," and, with implicit uses added to the explicit, "... the idea of equilibrium stands as the closest approximation to a general theory that can be found in the whole field" of social science.⁷

But social scientists have not restricted themselves to a single notion of equilibrium.⁸ From the variety of models, I select the following special case as appropriate for the study of deviance and control processes: a system is in equilibrium (regarding deviance) when, following an initial period in which the system contains a given potential for deviance and following a disturbance in the form of an increased potential which is manifest in deviance, the sanctions administered by controlling agents of the system in re-

* Revision of paper presented at the annual meeting of the American Sociological Society, August 1957. Some of the points arose in discussions with the late Andrew Henry of Vanderbilt University, Hope Leichter of the Jewish Family Service of New York, and Odd Ramsøy of the Institute for Sociology, Oslo, Norway.

¹ Kurt Lewin, *A Dynamic Theory of Personality*, New York: McGraw-Hill, 1935.

² Talcott Parsons and Edward A. Shils, editors, *Toward a General Theory of Action*, Cambridge: Harvard University Press, 1951, pp. 110-158.

³ V. Pareto, *Mind and Society*, New York: Harcourt, Brace, 1935, Vol. IV, pp. 1435-1442.

⁴ E. D. Chapple and C. S. Coon, *Principles of Anthropology*, New York: Holt, 1942.

⁵ George C. Homans, *The Human Group*, New York: Harcourt, Brace, 1950, pp. 421-423.

⁶ Parsons and Shils, *op. cit.*, pp. 190-233; Talcott Parsons, Robert F. Bales, and Edward A. Shils,

Working Papers in The Theory of Action, Glencoe, Ill.: Free Press, 1953, esp. pp. 99-103 and 111-161, and a revised form of the latter analysis in A. Paul Hare, Edgar F. Borgatta, and Robert F. Bales, editors, *Small Groups*, New York: Knopf, 1955, pp. 424-463.

⁷ David Easton, "Limits of the Equilibrium Model in Social Research," *Behavioral Science*, 1 (April, 1956) pp. 96-104.

⁸ Cf. *ibid.*

sponse to the deviance tend to reduce the potential so that it approaches its initial value.

If we let (A B) represent an ordered process with a given potential, *k* represent deviance, and *l* represent sanctions, then the following sequence illustrates one instance of a system in equilibrium:

A B A B A B *k* l A B A B A B.

This model can be applied appropriately and meaningfully only under very special conditions. I combine these conditions into an hypothetical system called the *simple system*. When a concrete case can be shown to correspond to it, the observation that the system is in equilibrium, as well as the observation that it is not, leads to further questions about its variant and invariant properties. When the concrete case departs from the simple one, however, the observation that the system is not in equilibrium may indicate only that we are not asking a reasonable question. The relevant question in this latter instance is not "What are the consequences of sanctions upon the potential for deviance?"—as asked in the model—but "What changes in the system are necessary to bring its conditions into line with the simple system, so that the issue of restoration to an initial potential is a problematical one?" In this way, the concept of the simple system is proposed as an intermediate one, standing between concrete behavioral systems and the equilibrium model. Before explaining its characteristics, certain terms should be defined, and two concepts concerning the boundaries of behavioral systems—the *domain of demands* and the *range of legitimate control*—should be introduced.

DEFINITIONS

Deviance. By *deviance* I refer to behavior which violates the norms of a person or a group and which is followed, or is expected to be followed, by sanctions from appropriate controlling agents. By *norms* I refer to a set of ideas as to how a person should behave—is indeed obliged to behave—under specified circumstances.⁹

For theoretical purposes the *context of deviance* may be either within the person-

ality, as illustrated by a private impulse followed by guilt or self-punishment,¹⁰ or within the group where overt behavior is expected to be followed by overt sanctions of one form or another. Each of these contexts has its own *controlling agent*. The function of the agent in the first case is to maintain internalized norms; in the second it is to maintain the group norms. Of course individuals, as members of groups, may serve in both capacities.

The Domain of Demands. For the most part when we interact with others we commit to them only a small part of ourselves—that part which belongs to *this* relationship, *this* group. Yet, while interacting we bring with us all of ourselves: we are subject to attachments, loyalties, and ties to other relationships; and we are subject to memories and unconscious processes which have their home elsewhere. Much of what we bring to the immediate situation is a stranger to it. This means that to protect the part of ourselves invested in this relationship we must manage a variety of internal demands. The total array of demands operating upon an individual, or upon all individuals organized in a group, I call the *domain of demands*.

Because of man's symbolic capacity, the nature of these demands and the number of groups, societies, persons, and so on, to which they refer seem to be limited only by the life histories of men. The demands may stem from within the immediate group, or they may refer to persons and to groups existing in other places at other times.¹¹ The demands may stem from conscious needs and wants, or they may exist in the unconscious. This is to say that they may be beyond the awareness not only of other group members but of the person himself. Known or unknown, and whatever their source, their totality in a given context I term the *domain of demands*.

I assume that deviance is causally connected to the configuration of demands in this domain. It is the manifestation of an attempt to manage intractable or funda-

¹⁰ Parsons and Shils, *op. cit.*, p. 142.

¹¹ Reference groups of course are one source of demands. See Robert K. Merton, *Social Structure and Social Theory*, Glencoe, Ill.: Free Press, 1957, Chapters 8 and 9.

⁹ This definition of norms follows Homans, *op. cit.*, pp. 121-125.

mentally incompatible demands. In this sense, it has an end and a direction. And though this purpose may not be obvious, I assume that it can be inferred from a knowledge of the underlying demands.

The Range of Legitimate Control. By this term I refer to the set of impulses and acts over which the controlling agents of a system have jurisdiction. Much as local, state, and federal governments each has its respective sphere of authority, controlling agents in social relationships and in groups have a defined realm of control. Similarly, their counterparts within the personality have legitimate control over some but not all internal processes. As the term legitimate implies, the range refers to the authority granted the agents by the norms of the system, not to the actual control exercised, which in the case of usurpation oversteps the legitimate range and in the case of negligence falls short of it.

Examples of spheres of authority in groups and parts of society are commonplace and need not be enumerated, but a brief comment may help to clarify the claim that personality systems possess the same type of boundary. When impulses and thoughts are repressed they are sent beyond the sphere of the governmental authority of the personality—the sphere that is legitimized by the internalized norms. Repressed impulses, like all unconscious processes, exist outside the range of legitimate control. Moreover, from this viewpoint, ego-defences, such as denial, isolation, and projection, function to keep certain matters outside the range.¹² In this sense, I suggest that all behavioral systems possess a boundary within which their controlling agents have jurisdiction and beyond which they do not.

THE SIMPLE SYSTEM

With these definitions and concepts in mind can we not picture a personality or a group where the problem of controlling deviance is a simple matter? I call such a personality or group a *simple system*. It has three essential characteristics.

The first of these is that, whatever the

extent of the domain of demands underlying deviance, the extent of the range of legitimate control is as great. This condition, for example, may be approximated within the personality through a knowledge of one's unconscious processes. As signals from conflicting and intractable demands are received, one works to interpret what is signified and to bring the underlying demands within the government of the personality. Knowing these demands and managing them become legitimate functions of the self. In similar fashion, this condition may be approached in the two-person situation as the therapist, by an ever-increasing knowledge of the demands impinging upon the patient, works indirectly through the patient to help him to become aware of these demands and to extend his management functions to include them. With success, the boundaries of the range of legitimate control spread toward the boundaries of the domain of demands.

As mentioned above, there are two agents of control in the group: one operating on behalf of selves, the others on behalf of the group norms. The jurisdiction of one may be limited while the jurisdiction of the other is extensive. For present purposes, however, this ratio is of secondary importance: *who* or *what* has control is distinct from the primary issue of whether or not control exists at all. Therefore, regardless of jurisdictional ratios, if the combined range of control includes all elements in the domain of demands, the situation meets the first condition of the simple system.

The second characteristic of the simple system concerns the immediate reaction to deviance, which is not to uphold automatically current values, norms, and beliefs. Rather, deviance poses the question: shall the norms be maintained by controlling this deviance or shall this behavior be viewed as innovation and the norms be changed accordingly? Or, as it is put by Parsons, Bales, and Shils: is the system to *maintain* its original state or is it to *learn* from this unexpected behavior?¹³

On the personal level, the question may be: Is this foreign thought, this strange impulse, to be repressed or is it to become part of a new state of integration yet to be

¹² Anna Freud, *The Ego and Mechanisms of Defence*, New York: International Universities Press, 1946, pp. 58-70.

¹³ Parsons, Bales, and Shils, *op. cit.*, Chapter 5.

worked out? The issue may confront groups as well. For example, as the child grows up, encountering new demands from within and from others and exhibiting new forms of behavior, the family, in effect, chooses either to enforce the established set of relationships, suited perhaps to a baby and nurturant parents, or to rearrange its normative structure so as to accommodate a youth and eventually an adult. In the simple system the issue is raised and faced, not ignored.

The third characteristic of the simple system is that once behavior is defined as deviance, the sanctions in the repertoire of controlling agents, if exercised, penetrate beyond the symptomatic act to the basic demands themselves. The silent member of the group who feels that a hostile word can destroy others and who, instead of being seduced into battle, is reassured to the point where he not only speaks but is not afraid, is one illustration. Another is the case when the interpretation of the therapist, in addition to eliminating a pattern of overt action, opens to the patient the possibility of comprehending and of organizing a set of conflicting demands. Or again, when the idea back of parental sanctions becomes incorporated as a strengthening ingredient within the child who by this means gains access to and management of forces otherwise beyond him.

To summarize, in the simple system the boundaries of the domain of demands are included within legitimate control boundaries, sanctions occur not automatically but only after behavior is defined as deviance rather than as innovation, and sanctions (when exercised) affect not only the symptomatic act but the basic demands. The simple system contains the conditions which must prevail in order to apply, appropriately and productively, the equilibrium model—the model of no deviance—deviance—sanctions—no deviance. This claim rests upon the following reasons:

1. When, contrary to the conditions in the simple system, demands underlying deviance are beyond the scope of legitimate control we may expect either routine application of sanctions, which by definition can not reach the demands, or application of more effective but illegitimate sanctions. In the first instance, there is no reason, beyond chance, to expect the sanctions to counteract or

neutralize the conflicting demands. Therefore, the question of reducing the probability of deviance has in the asking its answer: only by chance. Moreover, in the second instance, deviance by a member is followed by deviance on the part of an agent of control, introducing the possibility of a vicious circle. On the other hand, when conditions of the simple system prevail in the respect that underlying demands fall within the scope of control—when, in other words, control is not impossible—whether normative sanctions actually neutralize the conflict or whether they fail to do so becomes a meaningful question for both controlling agents and social scientists.

2. The second stipulation of the simple system would not be necessary if, on the one hand, internal and external demands upon behavioral systems were in fact constant and, on the other, norms could not be changed. However, since the body, for one thing, varies in the kinds and intensity of its demands, since adaptive exigencies of groups and of societies vary, since individual persons grow and learn, and since cultures change, provision should be made for the possibility that restoration to a previous state of non-deviance may be accomplished by changing the norms. Thus, even though the system is faced with irreducible conflicting demands which inevitably result in deviance, the issue of restoration remains relevant because the norms may be altered to accommodate would-be deviance as acceptable behavior, consequently reducing the potential. In these and similar circumstances, observations as to whether the norms are actually changed lead to further inferences about the system. But no such benefit derives from the question when it can be shown that the existing norms are rigid and applied automatically—none, that is, unless it can also be demonstrated that internal and external demands are in fact constant.

3. Finally, since sanctions which reach the symptomatic act but not beyond do not alter the system's potential for deviance, the pertinent question concerns the new manner in which demands will manifest themselves, not equilibrium. In contrast, since the simple system's repertoire contains sanctions which can affect the causal connections between demands and behavior, there is reason to

expect restoration, so that observations as to what sanctions are applied, in what manner, and with what consequences, again, are meaningful.

In short, unlike the situation in more complex systems, in the simple system the tendency toward restoration of a state of low potential for deviance is neither impossible, nor simply by chance, but a reasonable expectation over a wide range of circumstances.

DEPARTURES FROM THE SIMPLE SYSTEM

Major departures from the simple system render the issue of equilibrium inappropriate. This point may be developed by illustrating two types of departures likely to be encountered.

The first type is observed when the range of legitimate control excludes the domain of demands underlying deviant behavior. This may occur because the demands are unknown, as in the conspicuous case of the psychotic, but also in the case of the family whose child simply goes "out" to do "nothing," as well as the case of diplomatic negotiations when personal loyalties and cross-loyalties are far less understood than home instructions. In these instances, *some* of the demands making themselves felt are beyond the intelligence of all participants—certain forces are unknown to anyone in the system.

In other cases, even though the demands are thought to be known, they are inaccessible because the jurisdiction of controlling agents is limited. In attempting to treat the juvenile offender, for example, the court and its agencies may have formed a tentative diagnosis of the complicated internal and external forces confronting him, but are prevented by law from investigating and treating, beyond clearly defined limits, either the social world or the personal domain of the offender. His privacy and the privacy of his gang are protected by laws which insure individual rights for all of us.

In cases of this kind ignorance and inaccessibility complicate readjustment processes. So much so, in fact, that if controlling agents feel compelled to recapture a previous balance before they understand what is going on, their sanctions are not only likely to miss their mark but, by beclouding the true demands, by creating new ones, and by creating

an illusion that the matter is understood, to compound the state of ignorance and inaccessibility. Practically and theoretically, the expectation of restoring a previous low potential for deviance by means of existing sanctions is misplaced.

The second type of major departure occurs when the simple system contains subparts each of which is organized to maintain itself largely in its own terms.¹⁴ Such is the case of the foreman in his well known role conflict: his relation to the workers is one sub-system, his relation to management a second, and the relation between his role and himself as a person is another. Although the context is quite different, a similar complexity exists in the case of Bateson's "double-bind," illustrated by the relation between mother and child: if the child responds with affection the mother becomes anxious and withdraws; if the child withdraws, increased anxiety within the mother again leads to a demand for signs of affection.¹⁵ One sub-system focuses on the mother's need to be a good mother, the other on her desire not to be a mother, and these two conflicting sub-systems impinge upon the child. A similar complexity exists when, within the personality, feelings of what one *wants* to be are at war with feelings of what one *should* be.

These systems are complex, in the first place, because they contain sub-systems each with its own controlling agent; in the second place, because the sanctions proscribed by the norms of the sub-systems tend to intensify the conflict of demands originally leading to deviance. For example, in the case of the foreman, if management punishes a "working-man's foreman," if workers negatively sanction a "management-foreman," and if the foreman punishes himself for unsuccessfully maintaining some non-existing middle ground, the original conflict is intensified. This intensification is shown in the case of the double-bind for, if Bateson is correct, the built-in sanctions on the part of the child result in the creation of a new type

¹⁴ For a general discussion see Odd Ramsøy, "System and Subsystem: A Study in Sociological Theory," mimeographed, Oslo: Oslo University, 1958.

¹⁵ Gregory Bateson, D. D. Jackson, J. Haley, and J. H. Weakland, "Toward a Theory of Schizophrenia," *Behavioral Science*, 1 (October, 1956), pp. 251-264.

of communication and eventually of schizophrenic mental processes. The normal controlling activities guided by the norms within the sub-systems tend to increase, not to decrease, the probability of deviance. In this respect, whether or not sanctions will restore a previous low potential for deviance is not one of the more fruitful questions that might be raised.

THE COMPLEXITY OF BEHAVIORAL SYSTEMS

While we may use the set of conditions called the simple system in order to separate those concrete cases for which the equilibrium model is appropriate from those for which it is not, two questions remain: How likely, and under what conditions, are we to find concrete systems corresponding to the simple one? How are we to analyze deviance and control processes in more complex cases?

With respect to the first question—notwithstanding our limited knowledge of behavioral systems; indeed partly because of such limitations—I suggest that simple systems are rare. Moreover, as I shall indicate below, there are reasons to believe that departures result not from incomplete evolution, for example, but because positive forces operating to preserve other features of the system create a complex state and tend to keep it complex. Simple systems are rare because complicating factors are strong and, possibly, universal.

Are there not tendencies, for instance, which keep the domain of demands beyond the range of legitimate control? With respect to the personality, as noted above, psychoanalytical mechanisms describe how certain impulses, thoughts, and so on are excluded from the psyche's governmental jurisdiction. The mechanisms serve a positive need to maintain the integrity of the self. Impulses, once beyond the boundary of control, are kept there by resistance, which serves the same positive need. Active demands are kept beyond the range of direct control, not because they are weak or inconsequential, but by forces operating on behalf of the self. This discrepancy serves to preserve the self.

With respect to the group, the first obvious, though often overlooked, source of a similar discrepancy is that in groups when individuals meet they bring their individual defenses, thereby introducing into the group-

system sets of active demands beyond personal boundaries of control. The new aggregation of demands extends beyond the aggregation of realms of control and tends to be kept there by the members' defenses. Moreover, and quite apart from this source, the discrepancy is widened by two factors associated with the group as an organized unit. The first is the group's resistance to cultural change; the second is its resistance to adding to its responsibilities. By what it recognizes and does not recognize to exist, a group's culture selects the phenomena that can be acknowledged to be within the domain of demands. If not omniscient, it will fail to recognize certain demands and will tend to account for them in terms consistent with the accepted images, beliefs, and explanations. When these explanations are internalized members have a positive investment in ignoring demands not acknowledged by the culture and in explaining away those which contradict it. Various mechanisms are employed to keep unknown demands unknown and to deny incompatible ones; they serve the end of preserving the basic values and beliefs as they are.¹⁶ In performing this function, however, they create and maintain a gap between the domain of actual demands and the group's range of control.

The discrepancy may be widened even further by the group's conception of its responsibility. Paralleling the right to sanction is the obligation to control effectively, so that one consequence of acknowledging a demand as a cause of deviance is to extend the responsibility of controlling agents to cover the demand. Unless resources are unlimited in this respect, and in order to avoid negligence, groups tend to define, to limit, and to protect their realms of responsibility.¹⁷ Therefore, in general we can expect some demands to be beyond the range of control,

¹⁶ A pathological development in this direction is illustrated and discussed by Lyman C. Wynne, Irving M. Rycoff, Julian Day, and Stanley Hirsch in "Pseudo-mutuality in the Family Relations of Schizophrenics," *Psychiatry*, 21 (May, 1958), pp. 205-220.

¹⁷ Groups vary, of course, in their scope of responsibility. One way of characterizing these variations is by using Parson's pattern variables: a narrower scope is expected where role-expectations are universalistic and specific, for example, than where they are particularistic and diffuse. See Parsons and Shils, *op. cit.*, pp. 76-88.

not simply because they are unknown, nor because they are incompatible with cultural beliefs, but, from the group's point of view, because they are within the province of some other controlling body.

To the extent, then, that there is a need for groups to maintain their cultures and their spheres of responsibility, like individuals, they have a vested interest in surrounding themselves with a set of active demands which are either kept unknown or are defined as some other body's responsibility. As a part of an answer to our initial question, therefore, I suggest that while other things are equal, the stronger these protective factors the less likely a concrete system will correspond to the simple one.

Even though demands are accessible concerning jurisdiction, ignorance of the causes of deviance is another factor which prevents a system from becoming simple. The stringent requirement, it will be recalled, is that sanctions penetrate beyond the symptomatic act to the underlying demands. Under what conditions does this occur? Because of the incidence of deviance through time, its prevalence, and the positive part culture plays in proscribing sanctions,¹⁸ I believe that we can justifiably rule out the possibility that demands are neutralized or counteracted by some automatic means. Moreover, although we may be encouraged in finding a system with an effective repertoire, acquired through a long history of trial and error, we should not conclude that the system is simple—for unless it can be shown that demands are constant and will remain so, we have no reason to expect the repertoire to handle future contingencies. An alternative to trial and error, and to mechanical restoration, is an understanding, on the one hand, of the causal links between demands and deviance, and, on the other, between sanctions and demands. This alternative requires enough knowledge to form an accurate diagnosis, an expert prescription, and the freedom to administer the sanction effectively. I suggest that this alternative presumes more knowledge than we possess—more knowledge than

that of either controlling agents or social scientists—at least at present. In fact, it is largely from our realization of how little we know in this respect that increasing efforts are being made to discover the causes of deviance and the effectiveness of treatments. Until we know more, on what basis can we expect to find a simple system? Meanwhile, as our understanding improves, should we not expect a gradual increase in the number of simple systems and, consequently, more cases for which the equilibrium model is appropriate?

In summary, I have suggested that the following factors tend to counteract simplification tendencies: (1) the need to protect the integrity of the self; (2) the need to preserve the culture at its current level of integration; (3) the cost in extending the realm of responsibility; and (4) ignorance of the causal relations between demands, deviance, and sanctions. If, empirically, it is found that these complicating factors outweigh to a significant degree the simplifying ones, it means not only that the utility of the equilibrium model is severely limited but that behavioral systems, as a type, tend to be unstable. It means that they fail to maintain a boundary, as shown by the discrepancy between demands and range of control, and that lacking effective sanctions, they fail to absorb internal disturbances.¹⁹ To the degree that they are unstable, we are confronted with a difficult task in formulating general principles regarding deviance and control processes, for in an unstable system even though we observe an instance of restoration, for example, it provides no basis for anticipating a future instance nor for generalizing from one unstable system to another. For these reasons, empirical investigations of such properties as the domain of demands, the range of control, and the consequence of sanctions are important not merely in applying the equilibrium model but for our general understanding of behavioral systems.²⁰

¹⁹ Parsons and Shils, *op. cit.*, pp. 107–109.

²⁰ Cf. Lewin, *op. cit.*, p. 58, where in reference to studying the personality as a whole, he suggests that "Indeed, the concrete task of research will often consist precisely in the search for [the] determinative system, its boundaries and its internal structure."

¹⁸ Clyde Kluckhohn, "Culture and Behavior," in Gardner Lindzey, editor, *Handbook of Social Psychology*, Cambridge, Mass.: Addison-Wesley, 1954, Vol. II, Chapter 25.

TRANSITIONS FROM COMPLEX TO SIMPLE

Granting the argument so far, we reach an impasse. The simple system must prevail if we are to apply the equilibrium model fruitfully, yet this kind of system seems unlikely to exist. At best, behavioral systems seem to be complex; they may be inherently unstable. What course from the impasse might we follow? How, in general, might we conceive of processes of deviance and control?

My proposal is, first, that we postpone the issue of restoration, second, that in considering reactions to deviance we take into account more than sanctions, and third, that we use the conditions of the hypothetical simple system as that state which tends, or not, to be approached as a consequence of reactions to deviance. The central issue would shift from the question of whether or not systems are in equilibrium to the question of whether or not changes in the system, following deviance, bring the system closer to the hypothetical simple system, which we might expect to be in equilibrium. Simplification may be marked by a wide variety of changes in the concrete system. I call attention to only one of these, but do so because of its relevance to the two major departures described above, namely, the discrepancy between demands and the range of control and the existence of sub-systems.

Returning to the foreman, the double-bind, and unconscious conflict as illustrations of complex systems, and asking what will reduce the probability of deviance in these cases, it can be seen that reduction has nothing directly to do with the deviant events themselves, nor with an application of a specially devised sanction. Instead reduction calls for a reorganization of the system. This may take the form of negotiations between workers and management in the case of the foreman; it may be accomplished in the case of the double-bind by a redefinition of the situation to the effect that the mother is ill; it may be achieved in the case of an unconscious conflict between *wants* and *shoulds* by a strengthening, or an extension, of ego functions. The shortest path (however long it may be) to a lower probability of deviance is by way of developing a new system, not by applying new energy in administering sanctions in the

service of old norms. For this reason, we need to include in our model the addition, subtraction, and re-organization of diverse parts of the system, not merely sanctions. In so far as the negotiations between workers and management create an over-arching range of legitimate control, encompassing the originally conflicting demands, the system surrounding the foreman is simplified. Since the child's realization of the mother's illness is in Bateson's terms a meta-communication about the situation, or a comprehension of previously unrecognized connections between various demands, the intensity of the conflict caused by the mother's contradictory behavior diminishes and to this extent the system shifts toward a simpler state. And the personality system is simplified when the ego takes on new mediating functions in handling the intra-psychic conflict between *wants* and *shoulds*.

The first point illustrated by these changes is that when, in addition to sanctions, we incorporate into the model any modification which might result in either simplification or complication, our analysis involves the total structure of the system, how it is differentiated, how the different parts function, and how they are interrelated. The second point is the important distinction between simplicity of structure and simplicity of control, between the undifferentiated system and what I have defined as the simple system. They are not only different, but under certain circumstances they are inversely interrelated. In this way, because of the wider scope of functions it is able to perform, a personality with a highly differentiated ego is more likely to approach the simple state than one with a primitive ego; similarly, a highly differentiated group, particularly a group with various specialists who learn the demands, adjudicate conflicts, and consider the causes and effects of group events, is more apt to approximate the simple state than a group with a minimum division of labor. Although this correlation exists only within certain limits, it leads us to include the addition of roles and functions as one of the possible means by which systems shift from the complex to the simple type.

A case in point is illustrated by the history of the diagnosis and treatment of mental illness, which shows a progressive extension

and elaboration of roles: from the biologically oriented physician to the psychiatrist, the analyst, the social worker, the psychiatric social worker, the medical sociologist or anthropologist, and now in certain mental hospitals an increasingly elaborate and administratively complicated team. One aim of the team is to learn more about the demands pressing upon patients from many quarters, another is to extend therapeutic control beyond the single hour to "the other 23 hours of the day." As it has been realized that the demands underlying mental illness are illusive and extensive and that treatment may come from many quarters, new roles, functions, and so on have been added in the hope that the range of therapeutic control will encompass them. While these changes may complicate the system organizationally, from the viewpoint of deviance and control processes they can simplify it.

In terms of elegance and the power of an over-arching theory, the proposal to use the simple system as a set of conditions standing between concrete cases and the equilibrium model is a conservative move. But

I believe that it is a timely and appropriate one. It has an advantage in admitting complexities into behavioral systems—complexities increasingly substantiated by social science research—but at the same time allowing for those instances when a system might possibly be in equilibrium. In this way it may help avoid the illusion that systems must be in or near equilibrium to be of concern to a general theory of deviance and control processes. The proposal may in fact contribute substantially to the understanding of how far systems are from the state in which equilibrium is expected, on the one hand, and, on the other, of the factors that keep systems away from that state. Yet, it presents a challenge and a sense of direction to the empirical investigation of system processes for, like the equilibrium model, its application calls for improved techniques in identifying more precisely the domain of demands, the norms, the range of control, and the causal connections between sanctions and demands, as well as those between structural changes and the underlying demands.

CRIME, AGE, AND EMPLOYMENT *

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It was hypothesized that prior failure to find marked relationships between crime and economic conditions reflect the countervailing influences of an inverse relationship of juvenile criminality with unemployment and a direct relationship of offenses by adults with unemployment. Crude data for the United States, and somewhat more adequate data for Chicago, Boston, and Cincinnati, largely support the hypotheses. It is suggested that conversion to criminality, interpreted by the Merton "Social Structure and Anomie" paradigm, best explains these findings, and provides a needed complement to Sutherland's type of explanation for persistence in crime.

A REVIEW of past research makes it clear that no marked and consistent relationships have been established between overall or specific crime rates and economic conditions. In one of the most recent and exhaustive summaries of this litera-

ture, Vold concludes: "... assumptions involving either *positive* or *negative* relationships with economic conditions may be supported with some show of statistical significance. The obvious inference is that the general relations of economic conditions and criminality are so indefinite that no clear or definite conclusion can be drawn."¹

* This research was undertaken in connection with a larger study of the Effectiveness of the Federal Correctional System, financed by the Ford Foundation.

¹ George B. Vold, *Theoretical Criminology*, New York: Oxford University Press, 1958, p. 181.

HYPOTHESES

Despite Vold's discouragement, and despite notorious deficiencies of American crime statistics,² we had reason to believe that significant and stable relationships between crime and economic conditions might be demonstrated if the criminal population were appropriately differentiated by age and by offense. Our initial hypotheses, and their principal sources, were as follows:

Hypothesis 1. *The frequency of crimes committed by juveniles varies inversely with unemployment rates.* Several local studies of delinquency in the United States, covering the period around the depression of the 1930s, suggest that a decrease in delinquency occurred at that time.³ Plant observed such a decrease in Essex County, New Jersey, which he explained by the presumption that unemployment increased the time available to the father to be a companion to his children.⁴ We were impressed by this interpretation, as well as by evidence that concern for integration increases in small collaborative groups under threat,⁵ despite Komarovsky's allegation that the authority of the father often declines with unemployment.⁶ The apparent increases in delinquency during World War II and since 1948, provided further suggestion that parent-child contacts are the intervening variables in an inverse relationship between unemployment and juvenile delinquency; these rises in delinquency generally accompanied a simultaneous decline of male labor force unemployment and increase of married women in the labor force.

Emphasis on family relations as a primary factor in delinquency has been growing. This interpretation is often identified with the

view that the juvenile's basic personality or emotional adjustment is the product of his family relationships and, therefore, personality or emotional stability is the intervening variable between family relations and delinquency. Reckless, Glaser, and others, however, have presented research findings which suggest that enculturation may be the primary intervening variable. Like Plant, they view the family as the agency which provides the principal competition to delinquent peer groups in the inculcation of cultural or sub-cultural values.⁷

Our expectation that an inverse relationship between delinquency and unemployment would apply to all types of crimes committed by juveniles stems from the impression that delinquent sub-cultures promote a high valuation of deviant acts which are versatile and relatively non-utilitarian, and that a youth's internalization of deviant values is a function of the extent to which he and his parents live in different social and cultural worlds.⁸

Our initial hypotheses were expressed in terms of unemployment rates because this is an aspect of economic conditions which impinges most directly on the life of that portion of the population most liable to arrest, the lower socio-economic classes.

Hypothesis 2. *The frequency of property crimes committed by adults varies directly with unemployment rates.* Since adults differ from juveniles in the extent to which they are economically self-sufficient and are committed to legitimate occupations, it was anticipated that adult crime rates would vary directly with the inability to secure employ-

² Cf. Daniel Bell, "What Crime Wave?", *Fortune*, January, 1955, pp. 96-99, 154-56; Thorsten Sellin, as cited in Robert Wallace, "Crime in the United States," *Life* (September 9, 1957), pp. 47-70.

³ See, e.g., Thorsten Sellin, *Research Memorandum on Crime in the Depression*, New York: Social Science Research Council, Bulletin No. 27, 1937, pp. 54-56; David Bogen, "Juvenile Delinquency and Economic Trends," *American Sociological Review*, 9 (April, 1944), pp. 178-184.

⁴ James S. Plant, *Personality and the Cultural Pattern*, New York: The Commonwealth Fund, 1937, p. 141.

⁵ Cf. John T. Lanzetta, "Group Behavior Under Stress," *Human Relations*, 8 (1955), pp. 29-52.

⁶ Mirra Komarovsky, *The Unemployed Man and His Family*, New York: Dryden, 1940.

⁷ Walter C. Reckless, S. Dinitz, and E. Murray, "Self Concept as an Insulator Against Delinquency," *American Sociological Review*, 21 (December, 1956), pp. 744-746; Reckless, Dinitz and B. Kay, "The Self Concept in Potential Delinquency and Potential Non-Delinquency," *American Sociological Review*, 22 (October, 1957), pp. 566-570; Daniel Glaser, "A Reconsideration of Some Parole Prediction Factors," *American Sociological Review*, 3 (June, 1954), pp. 336-337.

⁸ Cf. A. K. Cohen, *Delinquent Boys*, Glencoe, Ill.: Free Press, 1955. For modifications of this view, see Gresham M. Sykes and David Matza, "Techniques of Neutralization: A Theory of Delinquency," *American Sociological Review*, 22 (December, 1957), pp. 664-670; and John I. Kitsuse and David C. Dietrick, "Delinquent Boys: A Critique," *American Sociological Review*, 24 (April, 1959), pp. 208-215.

ment. Presumably, this would be particularly true for property offenses. Knowledge that peak populations in many state penitentiaries occurred during the depression of the thirties buttressed this hypothesis, although increased use of probation and shorter sentences, rather than prosperity, may have affected the subsequent decline in penitentiary population.

If both of our hypotheses are valid, their opposing effects might account for previous failures to find marked relationships between crime and economic conditions.

TESTS BY UNITED STATES DATA

Since our hypotheses deal with the age of persons committing crimes, we needed data on criminals rather than the most complete index of the volume of crime: "crimes known to the police." Arrest data were preferred to conviction data for American criminals because the former are available for longer periods of time, and because offenses adjudicated in juvenile courts are not recorded as convictions. Arrestees, however, include some presumably innocent persons who are not convicted. We assume that the proportion of the latter group is sufficiently small or uniform through all our time, age, and offense categories that it does not radically distort the relationships which we are investigating.

Our first efforts to test the hypotheses used longitudinal analysis of variations in the volume of fingerprint arrests reported in F.B.I. *Uniform Crime Reports*. However, these statistics present problems, notably the large year-to-year differences in the total number of arrests reported by local police agencies for the F.B.I.'s compilations. These totals increased from 277,778 in 1932 to 793,671 in 1950, with considerable fluctuation between these years (for example, the 1946 "property" arrests totalled more than twice the number submitted in 1945). Such fluctuations required that we express the total number of arrests reported for each age group as a per cent of the total arrests reported for all ages. It would have been preferable to keep the arrest indices for different age groups more independent by expressing them as a percentage of the age-specific population in the communities submitting arrest data. Population data of this nature are not available, however, and the arrest figures re-

ported are an unknown and apparently sharply fluctuating per cent of total arrests.

One assumption in comparing arrest totals for different age groups is that any errors in reporting are uniformly distributed for all age groups. This assumption is known to be somewhat invalid since only the pre-1952 figures were based on fingerprints submitted to the F.B.I., and the movement against finger-printing juveniles is believed to have resulted after World War II in a decline in the proportion of arrests reported for juveniles, especially in 1951. When the F.B.I., in 1952, changed its system of age-specific data procurement from fingerprints to logs of arrests compiled at local police agencies, the number of juvenile arrests reported increased markedly. Because of these trends in the age-specific arrest figures, the hypotheses were tested only on F.B.I. statistics, from their inception in 1932 through 1950.

The age-specific arrest data could not be compared with unemployment rates for exactly corresponding groups because long-term arrest and unemployment figures are not reported for the same age categories. Accordingly, we compared age-specific arrest rates with both the total and the age-specific male civilian unemployment rates. As far as possible, the latter were broken down into the age groups with which the arrestees were believed to be in most direct economic competition.⁹ As Table 1 indicates, the patterns of relationships between arrests and both of these unemployment indices are similar. Since the index of total unemployment was less discriminating, the age-specific unemployment rates for the imperfectly matched age groups were then used for all other comparisons with arrest rates.

⁹ Total male unemployment rates are from *Handbook of Labor Statistics*, Washington: Department of Labor Bulletin 1016, 1950, p. 35. Age-specific male unemployment rates are from *Annual Reports of the Labor Force*, Series P-50, Washington: Department of Commerce. However, 1931-39 age-specific unemployment rates had to be estimated by assuming that their change from year to year was proportionate to the changes in total unemployment rates. The F.B.I. age-specific arrest figures do not separate arrests of males from arrests of females, but F.B.I. tabulations indicate that males constitute 92 per cent of persons arrested for property offenses, 85 per cent of arrestees for crimes against persons, and 90 per cent of misdemeanor arrestees.

The results of our first test of the hypotheses are presented in Tables 1 and 2. In Table 1 the first hypothesis is clearly verified. The second hypothesis is verified with respect to adults aged 19 through 34, but an unexpected inverse relationship was found between crime and unemployment for adults 35 and over, resembling the relationship predicted only for juveniles.

The correlation coefficients in Table 2 suggest that the relationships originally hypothesized for employment and property offenses hold equally well for major types of non-economic crimes. Arrests of juveniles for rape, intended homicide, and assault are inversely related to their unemployment, while a direct relationship is shown for arrestees between the ages 21 and 34. Again, a low negative relationship was found for offenders 35 years of age and over.

We considered two possible sociological explanations of the unexpected finding of an inverse relationship between felonious crimes and unemployment in the older age groups. The first, analogous to Durkheim's interpretation of increased suicide with prosperity, is

TABLE 1. CORRELATION COEFFICIENTS RELATING AGE-SPECIFIC U. S. PROPERTY OFFENSE ARRESTS,^a AS PER CENT OF ALL-AGES ARRESTS, TO PER CENT OF U. S. MALE LABOR FORCE UNEMPLOYED, 1932-1950

Arrestee Age Groups	Arrests versus Age-Specific Unemployment ^b	Arrests versus Total Unemployment ^b
(Labor Force Age Groups in Parentheses)		
All Ages	-.12	-.12
17 and under (14 through 19)	-.62**	-.56*
18 (14 through 19)	-.43	-.34
19 and 20 (20 through 24)	.41	.40
21 through 24 (20 through 24)	.51*	.48*
25 through 34 (25 and over)	.72***	.74***
35 through 44 (25 and over)	-.26	-.26
45 and over (25 and over)	-.64**	-.64**

^a Arrests for Larceny, Burglary, Robbery, and Auto Theft.

^b Significance of difference from $r = 0$: *.02; **.01; ***.001.

TABLE 2. CORRELATION COEFFICIENTS RELATING AGE-SPECIFIC U. S. ARRESTS FOR CRIMES AGAINST PERSONS^a AND FOR PERSONAL DISORGANIZATION MISDEMEANORS,^b AS PER CENT OF ALL AGES ARRESTS, TO PER CENT OF U. S. MALE LABOR FORCE UNEMPLOYED, 1932-1950

Arrestee Age Groups	Crimes Against Persons ^c	Misdemeanors ^c
All Ages	-.74***	-.80***
17 and under	-.62**	.02
18	-.71***	.32
19 and 20	-.23	.61**
21 through 24	.31	.70***
25 through 34	.77***	.96***
35 through 44	-.32	-.76***
45 and over	-.18	-.84***

^a Arrests for Intended Homicide, Assault, and Rape.

^b Arrests for Drunkenness, Disorderly Conduct, and Vagrancy.

^c Significance of difference from $r = 0$: *.05; **.01; ***.001.

that personal disorganization or anomie develops among older people because of their inability to adjust to increased economic means. This interpretation seemed somewhat validated by the finding that arrests for "personal disorganization" misdemeanors were negatively related, and strongly so, to unemployment for the older age groups, but not for juveniles. We also speculated that disorganization of older persons might increase with prosperity because parents are needed by adult children during depressions, but lose parental functions during prosperity when their older offspring can be more self-sufficient.

An obvious alternative explanation for both the juvenile and old-age inverse relationships is that they are artifacts resulting from our indication of United States age-specific arrest trends by shifts in the proportion of total arrests contributed by specific age groups. Any marked change in arrests for one age group, expressed as a percentage of all arrests, would produce an inverse change in the percentage contributed by other age groups. Thus a marked positive relationship between unemployment and arrests for the middle-age ranges would produce, as artifacts, negative relationships for the other age ranges, or *vice versa*. To eliminate this possibility, we needed data in which crime rates for different age groups would be inde-

pendent of each other, which we sought in municipal reports.

TESTS BY MUNICIPAL DATA

A survey of municipal reports revealed that sex and age-specific arrest rates, differentiated by offense, had been published for several decades in fairly consistent form by police departments of three major cities: Chicago, Cincinnati, and Boston. The Chicago figures were somewhat suspect because of occasional odd fluctuations. The Cincinnati series, dating from 1935, inspired confidence because of the outstanding reputation of its public administration. We inferred that Boston data were punctiliously compiled by a highly compulsive bureaucracy because archaic language forms, and other details of annual presentation, were remarkably unchanged over a fifty-year period.

Unfortunately, we could obtain only age- and sex-specific population data for these cities for census years. A linear interpolation was employed to estimate the age-specific male population for inter-censal years. The age-specific arrests could then be expressed as a per cent of the corresponding age population. This linear interpolation somewhat distorts actual trends, however, particularly in the 1940-50 decade, when the annual change in municipal age-group population certainly was not constant. Some further imperfection of the data as a test of our hypotheses lay in the need to use the national age-specific unemployment rates, for municipal rates were available only for recent years

(and regrettably, United States pre-1930 unemployment data are not available).

The relationships between these independent arrest rates for different age groups in the three municipalities, and United States unemployment rates, are presented in Table 3. They consistently confirm our prior conclusions with respect to our second hypothesis: that property theft and unemployment are positively related for the middle-age ranges. Only the Chicago figures fail to repeat our earlier finding, using national data, of a similar direct relationship between unemployment and both crimes against persons and the "personal disorganization" misdemeanors. The differences among these three cities in the overall relationship between arrests and unemployment (the "All Ages" findings) make any similarity in the age-specific findings particularly noteworthy.

The municipal evidence is less clear-cut in supporting the first hypothesis, which predicted an inverse relationship between all juvenile offenses and unemployment. This hypothesis is buttressed for the 10-17 age categories in the Boston data, but with negative correlations that are not significantly below zero, although significantly below the positive correlations for the older age groups. The 18-20 age group in Boston follows the adult pattern of increased property offenses with unemployment, but shows an inverse pattern on other offenses. The Chicago and Cincinnati arrest reports do not differentiate ages younger than 20 throughout the period covered. Significant negative relationships

TABLE 3. CORRELATION COEFFICIENTS^a RELATING ARRESTS OF MALES IN BOSTON, CINCINNATI, AND CHICAGO BY TYPE OF OFFENSE, TO PER CENT OF U. S. MALE LABOR FORCE UNEMPLOYED, 1930-1956^b

Arrestee Age Group	Property Theft			Crimes Against Persons			Misdemeanors		
	Boston	Chicago	Cincinnati	Boston	Chicago	Cincinnati	Boston	Chicago	Cincinnati
All Ages	.14	.50**	.77***	.56**	.03	.63***	.78***	-.02	.85***
10-17	-.23			-.42			-.35		
18-20	.56**			-.35			-.29		
20 and under		-.01	.28		-.39*	-.79***		-.32	-.69***
21 through 24	.62***	.53***	.87***	.60***	.16	.61***	.24	.30	.81***
25 through 34	.77***	.61***	.93***	.58**	.08	.56**	.89***	.21	.90***
35 through 44	.69***	.76***	.92***	.59**	.10	.79***	.77***	-.03	.91***
45 and over		.83***	.78***		.35	.78***		-.16	.82***
45 through 54	.26			.58**			.55**		
55 and over	.36			.50**			.77***		

^a Significance of difference from $r = 0$: *.05; **.01; ***.001.

^b All Cincinnati arrest figures and 10-17 age group arrest figures for Boston are for 1935-1956 only.

between crimes of this youngest age group in these two cities and unemployment were found for crimes against persons and for misdemeanors, but not for property offenses. However, property offense arrests of those 20 and under in Chicago and Cincinnati were markedly less related to unemployment than were arrests of adults, and there is a continuous increase in this relationship from the 20 and under to the somewhat older groups. This suggests that negative correlations, such as those yielded by the Boston data, might also have been procured for property offenses if as fine an age breakdown as that procured for Boston were available. Of course, a major portion of youth over 17 are in the job market, like adults. In general, the consistent pattern of difference between the relationship of crime and unemployment for the youngest age group and this relationship for the age groups between 20 and 44 is more impressive support for our first hypothesis than the statistical significance of correlation coefficients for these data.¹⁰

The unexpected finding, discussed above of a negative relationship between older age offenses and unemployment, was not repeated with the municipal figures. Indeed, the three cities and the various offense categories investigated are inconsistent with respect to correlations between older age offenses and unemployment. A possible partial explanation of these inconsistencies is the marked differentiation between the cities in fluctuation of age-specific population composition. Between 1940 and 1950 the population 45 and over of Cincinnati increased 95 per cent and that of Chicago increased 53 per cent, while there was only a nine per cent increase in this age-category for Boston. The distortion from our linear interpolation assumption of a 9.5 per cent increase from the 1940 figures each year in Cincinnati may be particularly defective; we are safest in linear interpolation with the relatively stable Boston population. Data on the racial and cultural background of the new older-age population in Cincinnati and Chicago might help to explain the irregularities in our findings for these two cities. The deficiencies in the source data, as well as the curvilinear rela-

TABLE 4. MEANS AND STANDARD DEVIATIONS OF ANNUAL PER CENT U. S. AGE-SPECIFIC MALE LABOR FORCE UNEMPLOYED AND BOSTON ANNUAL AGE-SPECIFIC MALE ARRESTS FOR PROPERTY THEFT AS PER CENT OF BOSTON AGE-SPECIFIC MALE POPULATION, 1930-56

A. U. S. UNEMPLOYMENT		
Age Group	Mean	Standard Deviation
*14-19	12.1	6.9
20-24	14.8	10.3
25 and over	7.6	6.1
B. BOSTON PROPERTY OFFENSE ARRESTS		
Age Group	Mean	Standard Deviation
*10-17	2.86	.54
18-20	1.45	.27
21-24	1.63	.61
25-34	1.41	.34
35-44	1.04	.14
45-54	.59	.10
55 and over	.20	.04

* Youngest age group figures are for 1936-56.

tionships between age and the arrest-unemployment correlation coefficients, deterred us from partial regression analysis, using age as a third variable. We must accept an artifactual explanation for the inverse correlations in our national findings for the older age categories in the absence of data required for a more refined and precise analysis.

That conditions other than economic circumstances are particularly relevant to an understanding of trends in juvenile arrest rates is suggested by the fact that these rates are more independent of unemployment conditions than are the arrest rates of other age groups.

Some of the details of the data from which our computations were made are illustrated in Table 4. It is noteworthy that there is much more variance in unemployment than in property-offense arrest rates, and that older age property-offense arrest rates are particularly low and stable. The higher variance of property-offense arrest rates for the 21 to 34 age range, and their pronounced direct relationship to unemployment rates, would produce a spurious inverse relationship of the relatively stable older age arrest rates and unemployment, if all age-specific arrest rates were expressed as percentages of total

¹⁰ Cf. Hanan C. Selvin, "A Critique of Tests of Significance in Survey Research," *American Sociological Review*, 22 (October, 1957), pp. 519-527.

arrests. For crimes against persons, of which there were relatively few, and for misdemeanors, age differences in rates and variance were less pronounced.

CONCLUSIONS

Despite large deficiencies of available data, we have presented evidence which suggests that more pronounced and consistent relationships between crime and economic conditions than were reported by previous studies can be obtained by using age-specific statistics. Consistent support was presented for the hypothesis that adult crime rates vary directly with unemployment, particularly rates of property offenses by persons of 20 to 45 years of age. Less conclusive—but appreciable—evidence was presented for the hypothesis that juvenile crime rates vary inversely with unemployment.

The finding that economic conditions have markedly contrasting relationships to crime for different age groups is consistent with Parson's observations on the importance of age and sex role differences in American society.¹¹ It appears that unemployment is closely and directly related to criminality for males only when they are most strongly oriented to occupational stability or mobility.

Most criminology textbooks cite the low relationship of economic conditions to non-age-specific criminality as a basis for refuting Marxian explanations for crime. Our findings could be used by Marxists to ascribe criminality, at least in young adults, to the economic insecurity inherent in capitalism. A more parsimonious explanation, however, in the sense that it fits a variety of deviant behavior, is provided by Merton's paradigm which views crime as an innovating reaction to anomie, and stipulates the conditions for its priority over alternative reactions. This paradigm not only may be employed to account for the young adult's recourse to criminal means for economic ends when faced with unemployment, but also has been used to explain non-economic aspects of juvenile delinquency.¹²

As economic insecurity is reduced, we may still expect criminality to be associated with status insecurity or other sources of anomie. The generality of this phenomenon under quite different economic systems is suggested by Deaton's argument that post-World War II juvenile delinquency in the U.S.S.R. is distinctly associated with the anomie of the children of middle-class Russian families. The offspring of Soviet managers and professionals, reared in permissive family homes rather than factory nurseries, are more "on their own" than children of the Soviet proletariat. They are particularly anomic and prone to develop delinquent sub-cultures in late adolescence when they must suddenly shift to the regimented life which lower-class Russian children have experienced from infancy.¹³

If the Merton paradigm is to be applied to diverse types of criminality, what of the dominant American sociological explanation for crime which views it as a consequence of differential association or identification with criminals?¹⁴ It seems to us that the two are complementary. The Merton paradigm provides a needed explanation for conversion from a criminal to a non-criminal "way of life," and *vice versa*. Changes in the opportunity structure, as a result of job market fluctuations, alter the relative accessibility of legitimate and illegitimate means.¹⁵ A more detailed psychological analysis of this conversion process also is required, such as Cressey's interpretation of rationalization as

130; Richard A. Cloward, "Illegitimate Means, Anomie, and Deviant Behavior," *American Sociological Review*, 24 (April, 1959), pp. 164-176; also, forthcoming articles by Cloward and L. E. Ohlin in which different types of delinquent sub-cultures are interpreted as alternative solutions to problems of anomie.

¹³ Robert B. Deaton, "Postwar Juvenile Delinquency in the Union of Soviet Socialist Republics," unpublished M.A. Thesis, University of Illinois, 1955.

¹⁴ Cf. E. H. Sutherland and Donald R. Cressey, *Principles of Criminology*, Philadelphia: Lippincott, 1955, Chapter 4; Daniel Glaser, "The Sociological Approach to Crime and Corrections," *Law and Contemporary Problems*, 23 (Autumn, 1958), pp. 683-702.

¹⁵ Cloward, *op. cit.*; R. K. Merton, "Social Conformity, Deviation, and Opportunity Structures: A Comment on the Contributions of Durkheim and Cloward," *American Sociological Review*, 24 (April, 1959), pp. 177-189.

¹¹ Cf. Talcott Parsons, "Age and Sex in the Social Structure of the United States," *American Sociological Review*, 7 (October, 1942), pp. 604-616.

¹² Robert K. Merton, *Social Theory and Social Structure*, Glencoe, Ill.: Free Press, 1957, pp. 121-

the essential component of motivation in embezzlements committed by previously conventional persons.¹⁶

Once a person has been involved in criminality, whether because of early enculturation in delinquency or later conversion, and particularly when he has been arrested, committed to penal institutions, and released with the stigma of a criminal record, his criminal norms and his access to criminal means have been reinforced by his differential association and identification. Thus Merton's sociology may be needed to explain conversion to crime, while Sutherland's crim-

inology may be applied within Merton's frame of reference to explain persistence in crime.

It is hoped that others may find more adequate data for testing our hypotheses, and refining them, possibly making qualifications for females and for persons of distinct cultural background or socio-economic status. Perhaps tests could be conducted more satisfactorily in European countries or elsewhere where national police and employment control produce more adequate crime and unemployment statistics than we have in the United States. However, since crime may be a price we pay for freedom from governmental restraints, our hypotheses may not be as applicable to such countries as to the United States. But if our interpretation of the findings is correct, the hypotheses will hold increasingly wherever there is growth in industrialization, urbanization, and employment of women outside the home. All of these conditions truncate traditional home functions, but in a manner which varies with age and with economic conditions.

¹⁶ Donald R. Cressey, *Other People's Money*, Glencoe, Ill.: Free Press, 1953; and Cressey, "Application and Verification of the Differential Association Theory," *Journal of Criminal Law, Criminology and Police Science*, 53 (May-June, 1952), pp. 43-52. For a more generalized view of the verbal basis for human motivation, see George A. Kelly, *The Psychology of Personal Constructs*, New York: Norton, 1955; and Kelly, "Man's Construction of His Alternatives," in Gardner Lindzey, editor, *Assessment of Human Motives*, New York: Rinehart, 1958, Chapter 2.

RESEARCH REPORTS AND NOTES

MIGRATION AND MENTAL ILLNESS: A NEW LOOK

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In spite of the fact that considerable literature exists concerning the relationships between migration and mental illness,¹ this body of work is characterized by inconclusive results.² The recent study by Malzberg and Lee shows that the rate of first admissions of foreign born to mental hospitals in New York State (from 1938 to 1941) does not greatly exceed the rate for the native born once the proper corrections are made for age differentials in the two populations.³ This finding does not completely support the earlier conclusions of Odegaard⁴ and of Faris and Dunham,⁵ which indicate a pronounced higher rate of first admissions among the foreign born. Odegaard, however, cites a trend marked by a decrease in the differences between the rates of foreign born and natives.

Studies dealing with the relationship between interstate migration and rates of first admissions to mental hospitals are even more inconclusive. Malzberg found that the crude (uncorrected for age differences) rates for white and Negro interstate migrants from 1929 to 1931 were, respectively, 3.5 times higher⁶ and 4.7 times higher⁷

than those for the corresponding non-migrant populations in New York State. Although Malzberg recognizes the limited value of drawing conclusions from statistics uncorrected for age differences in the populations, he believes the large discrepancy noted above can not be accounted for completely on the basis of age distributions. Owing to changes in the reporting of the 1940 decennial census, Malzberg was able to reexamine this problem. After making appropriate corrections for age, figures on first admissions from 1939 to 1941 in New York State indicate that rates for native born white migrants to the state exceeded the rates for persons born in the state by 58 per cent for males and 73 per cent for females. For the non-white population, the excess of the rates for the migrants varied from 50 per cent for males to 53 per cent for females.⁸

These significant results are not supported by the findings of Tietze, Lapouse, and Hollingshead and Redlich.⁹ These studies show no direct relationship between interstate migration and the amount of mental disorder in the population. In spite of considerable methodological differences among these studies, the findings leave the problem of the relationship between interstate migration and diagnosed mental disorder in a hypothetical state. This paper reexamines the question of first admission rates to mental hospitals of interstate migrant and non-migrant Negroes in Pennsylvania.

Among Certain Population Groups in New York State," *Journal of the American Statistical Association*, 31 (September, 1936), p. 546.

⁷ Malzberg, "Migration and Mental Disease Among Negroes in New York State," *American Journal of Physical Anthropology*, 21 (January, 1936), pp. 107-113.

⁸ Malzberg and Lee, *op. cit.*, p. 75.

⁹ See, respectively, C. Tietze, P. Lemkau, and M. Cooper, "Personality Disorder and Spatial Mobility," *American Journal of Sociology*, 48 (July, 1942), pp. 29-39; Rema Lapouse, Mary A. Monk, and Milton Terris, "The Drift Hypothesis and Socio-economic Differentials in Schizophrenia," *American Journal of Public Health*, 46 (July-December, 1956), pp. 979-986; and August B. Hollingshead and Frederick Redlich, *Social Class and Mental Illness*, New York: Wiley, 1958, pp. 244-248.

¹ See Benjamin Malzberg and Everett S. Lee, *Migration and Mental Disease*, New York: Social Science Research Council, 1956.

² H. B. M. Murphey, "Migration and Mental Disease," *The Milbank Memorial Fund Quarterly*, 36 (January, 1958), pp. 86-89.

³ Malzberg and Lee, *op. cit.*, p. 58.

⁴ O. Odegaard, "Emigration and Insanity, A Study of Mental Disease Among the Norwegian Born Population of Minnesota," *Acta Psychiatrica et Neurological*, Supplementum 4, Copenhagen, (1932).

⁵ Robert E. L. Faris and H. Warren Dunham, *Mental Disorders in Urban Areas*, Chicago: University of Chicago Press, 1939.

⁶ Benjamin Malzberg, "Rates of Mental Disease

TABLE 1. DISTRIBUTIONS OF FIRST ADMISSIONS TO STATE HOSPITALS AND CENSUS POPULATION BY NATIVITY AND AGE

Age	MALE NEGROES							
	S-N		N-N		Native		Total Cases	
	Patients	1950 Census	Patients	1950 Census	Patients	1950 Census	Patients	1950 Census
20-29*	38%	46%	17%	4%	45%	46%	305	48890
30-39*	52	68	14	4	34	25	278	46885
40-49*	67	74	7	3	26	19	171	43015
50-59	77	78	5	3	18	15	150	31390
60-69	74	78	4	1	22	15	114	15840
70 plus	80	76	2	4	18	17	137	7210
Total*	59%	67%	10%	4%	31%	27%	1155	193230
Median Age	47.1	43.1	31.9	38.4	34.2	32.6		
Age	FEMALE NEGROES							
	S-N		N-N		Native		Total Cases	
	Patients	1950 Census	Patients	1950 Census	Patients	1950 Census	Patients	1950 Census
20-29***	43%	48%	7%	4%	50%	45%	224	59795
30-39*	62	68	11	3	27	25	204	57445
40-49**	68	74	8	3	24	20	155	44300
50-59	74	77	6	3	20	17	96	28670
60-69	77	77	0	3	23	17	79	16075
70 plus	82	73	1	5	17	19	102	8160
Total	63%	67%	7%	4%	30%	28%	860	214445
Median Age	44.7	40.8	36.1	36.9	33.0	32.0		
Age	MALE AND FEMALE COMBINED							
	S-N		N-N		Native		Total Cases	
	Patients	1950 Census	Patients	1950 Census	Patients	1950 Census	Patients	1950 Census
Total*	61%	68%	9%	4%	30%	28%	2013	407675

* Using X^2 , the difference between the distribution of the patient population and census figures is significant at less than .001.

** Using X^2 , the difference between the distribution of the patient population and census figures is significant at less than .01.

*** Using X^2 , the difference between the distribution of the patient population and census figures is not significant with a probability value at less than the .12 level.

PROCEDURE

Two thousand thirteen first admissions of Negroes to Pennsylvania state psychiatric hospitals during the 1951-1956 period were selected for study. The sample includes only those for whom ethnic origin is clearly Negro and for whom a state or city is entered on the record as the patient's place of birth. Sex, age, and diagnosis were also recorded for evaluation. The sample was subdivided into migrants born in southern United States (S-N), those born in other states excluding Pennsylvania (N-N), and those born in Pennsylvania (Natives). "Southern United States," as defined by the 1950 United States Census, includes: Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.¹⁰

The "normal" control population was based

¹⁰ Washington, D. C. was also included in the southern region.

on the figures reported in the 1950 census for Pennsylvania. The population was also subdivided into S-N, N-N, and Native categories, defined in the same way as the patient population. Information was available on the sex and age distribution of each division.

Since many diagnostic groupings were small, thus precluding statistical evaluation, the categories were collapsed into eight groupings not including "unknown." The Chi-square method was used to test for statistical significance, except in the case of the comparison of female S-N and Native patients with the N-N category because of the latter's insufficient cell frequencies.

RESULTS

In comparing the migrant and non-migrant distributions in the psychiatric and census populations, the difference is significant at less than the .001 level of confidence. In the patient population, the S-N category is under-repre-

sented while both the N-N and Native categories are over-represented. When the patient population is subdivided into males and females, the distribution of the sexes differs significantly at less than the .04 level of probability. For this reason, the data are presented separately for males and females in Tables 1 and 2.

To determine whether or not the difference between the patient and census populations is due to concentrations at specific age intervals, the data were subdivided into several age intervals. Table 1 shows that, for males, the distribution differences between the patient and census populations are significant for the ages 20-29, 30-39, and 40-49 at less than the .001 level. In each of these intervals, the S-N group is under-represented and the N-N group over-represented. The Native group is over-represented only in the 30-39 and 40-49 age intervals. Table 1 also shows that, for females, the probability values for the three age intervals mentioned above are less than .12, .001, and .01, respectively.

Since migrants move to new communities after spending a certain time in their native communities, one would expect the migrant population to be older than the Native. Table 1 shows that in both the census and patient populations, the median ages of the migrant divisions are higher than those of the Native, regardless of sex. The one exception is the N-N male patient category.

Table 2 shows that, for diagnosis, the S-N male patient population differs from the N-N and Native patient populations at less than the .001 level of confidence. The S-N patients show relatively more "Chronic Brain Syndrome with Cerebral Arteriosclerosis" and "Chronic Brain Syndrome—Other." There are also relatively less "Schizophrenia and Paranoia" and "Psychoneuroses—Personality Disorders" than among either the N-N or Native patients. The relatively high incidence of "Chronic Brain Syndrome with Cerebral Arteriosclerosis" apparently reflects the older age of the S-N population. Table 2 also shows that the difference between

TABLE 2. DIAGNOSIS BY SEX AND NATIVITY

DIAGNOSIS	MALES		
	S-N	N-N	Native
Chronic Brain Syndrome with Cerebral Arteriosclerosis	25%	7%	10%
Chronic Brain Syndrome—Other	17	7	9
Acute Brain Syndrome	11	6	16
Mental Deficiency	2	6	2
Affective Psychosis	1	2	2
Schizophrenia and Paranoia	37	59	44
Other Psychoses	2	1	3
Psychoneuroses—Personality Disorders	3	8	10
Unknown	2	2	4
	100 N = 684	100 N = 97	100 N = 357
DIAGNOSIS	FEMALES		
	S-N	N-N	Native
Chronic Brain Syndrome with Cerebral Arteriosclerosis	24%	5%	12%
Chronic Brain Syndrome—Other	9	5	9
Acute Brain Syndrome	9	5	12
Mental Deficiency	1	5	1
Affective Psychosis	5	5	6
Schizophrenia and Paranoia	44	66	49
Other Psychoses	4	9	3
Psychoneuroses—Personality Disorders	3	0	6
Unknown	1	0	2
	100 N = 540	100 N = 58	100 N = 259

Using X^2 , the significant p values for each pair of groups are:

S-N w NAT. — Males < .001

S-N w N-N — Males < .001

NAT w N-N — Males < .02

S-N w NAT. — Females < .001

The X^2 could not be used in comparisons involving the female N-N group because of insufficient frequencies in the cells.

the N-N and the Native populations is significant at less than the .02 level—the difference apparently attributable to the high incidence of schizophrenia and low incidence of "Acute Brain Syndrome" disorders (primarily alcoholism) in the N-N group.

Table 2 indicates further that, for females, the S-N and Native patients differ significantly, with a p of less than .001. With the exception of "Chronic Brain Syndrome—Other," the distribution of psychiatric disorders in these two female populations are the same as for the corresponding male categories. Although no test of significance was made of the difference between the S-N and N-N female populations, the distributions hold as well for males except for "Psychoneuroses—Personality Disorders." The differences in the distribution of psychiatric disorders between the N-N and Native females are the same as for the corresponding male populations.

DISCUSSION AND CONCLUSIONS

The data clearly indicate that the southern Negro migrant population is under-represented and that both the northern migrants and Natives are over-presented in the statistics on first admissions. These results remain evident when comparisons are made according to sex and age groupings. The exception to this generalization is the 70 and over age population. This exception may be explained as due to possible differences in the actual levels of psychopathology in the population, or as an artifact of differences in the economic resources or family cohesion, or both, between southern and northern Negroes. Southern Negroes, having less financial resources and fewer family ties, possibly are more likely to be hospitalized than Northern Negroes.

Generally, the distribution patterns of all three groupings in the patient and census popu-

lations are similar for males and females. It is noteworthy, however, that southern females show more mental illness than southern males. This suggests the possibility that the frequently noted tendency of southern migrant families to be "matricentered" places a considerable burden of responsibility and stress on the southern married female in the northern urban situation. This interpretation would also explain the fact that differences among the females in the 20-29 age group (that is, where marriage is comparatively less frequent) are not statistically significant.

The data presented in this paper differ strikingly from those reported by Malzberg for an earlier period in New York State. It may be that Negroes migrating to the North during the war and post-war period are less prone for constitutional reasons to mental illness than those who migrated in the past. It is more probable that historically different socio-economic conditions and motivational factors affect the selection of migrants. An alternative—or, perhaps, complementary explanation would give more weight to changes in social role that influence the relative experience of psychological stress of the southern migrants and northerners in the northern urban environment. An interesting hypothesis is that the discrepancy between level of aspiration and goal attainment for Native Negroes is larger than that for migrants, leading to greater stress among the former.¹¹

Our data tend to support those investigations that cast doubt on the psychopathogenic qualities of the interstate migration experience *itself* as an important factor in mental disorders. This study, we believe, points to the need for going beyond gross epidemiological enquiries and suggests hypotheses for more refined research.

¹¹ The authors are currently engaged in research designed to investigate this hypothesis.

COMMUNICATIONS

THE "ACHIEVEMENT SYNDROME" AND NEGRO AMERICANS

To the Editor:

Bernard C. Rosen's article, "Race, Ethnicity, and the Achievement Syndrome," (*American Sociological Review*, 24 [February, 1959], pp. 47-60) is an interesting examination of the differences in achievement orientation between six different ethnic and racial groups in North-eastern United States. In one sense, this study represents an important quantitative validation of some of the qualitative ethnographic and historical studies to which Dr. Rosen refers. For this reason, it may also be possible to throw some additional light on the quantitative findings by referring back to essentially non-quantitative considerations.

Rosen expected to find that the achievement orientation of Jews, Greeks, and white Protestants would be highest; that Southern Italians and French-Canadians would rank lower; and that Negroes would rank lowest of all. My main interest is in his apparently contradictory findings for the Negro group. While the Negroes do rank lowest on achievement motivation and vocational aspiration, they rank third on independence training, fourth on achievement values, and fourth on educational aspiration. Why should there be such differences? Part of the explanation may be due to an unwarranted assumption that the questions asked are getting at components of the "achievement syndrome." For example, independence training may be emphasized because of circumstances which actually make it necessary for a child to take care of himself early, quite aside from the question of achievement motivation. Similarly, an individualistic orientation may reflect the need for an individual to look out for himself, quite aside from the question of achievement values.

It is worth elaborating the point on achievement values, where Rosen's "prediction for the Negroes proved to be entirely wrong." (p. 57) Two of the seven questions that respondents were asked refer to their "Individualistic-Collectivistic Orientation"—one question dealing with an individual's ties to his parents and the other [with] his work organization. Rosen himself expresses doubts about the latter point on the basis of what William H. Whyte, Jr. has written about "the organization man." But it is also doubtful whether an individual's willingness to

separate from his parents is necessarily a reflection of achievement values which encourage social mobility. It may also be a realistic reaction to difficult economic conditions in that it makes it easier for the individual to be geographically mobile in order to get any kind of job. I wonder, therefore, whether the results would differ if the groups were compared on the other five questions alone, which are perhaps better indicators of achievement values?

A second possibility which may partly explain the apparently contradictory results on the Negroes is that the respondents may have reacted defensively to some of the questions they were asked by the (presumably white) interviewers. Some of the questions which try to get at achievement values seem to be too close to certain Negro stereotypes. For example, Item 7 reads: "Nowadays with world conditions the way they are the wise person lives for today and lets tomorrow take care of itself." (p. 56) Some of the Negroes may be reacting to the implied irresponsibility of the question, and perhaps this accounts for the unexpected results.

Since I am presently analyzing data on lower-class Negro families and values in Trinidad, I would be interested in Rosen's further comments on his material, and especially on the "achievement values" section, where the data on the Negroes seem least satisfactory. This is not meant to detract from the other findings, where his hypotheses about ethnic differences were borne out; nor does what I say and suspect about the "apparent contradictions" completely rule out the possibility that he has come upon real contradictions that have other explanations than the two I mention here.

HYMAN RODMAN

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REPLY TO RODMAN

To the Editor:

On closer inspection the discrepancies between predictions and findings on the Negroes seem not to be quite the "contradictions" Dr. Rodman believes them to be. The fact that the Negroes score lowest on achievement motivation despite their median score on independence training is not surprising. Independence training had been hypothesized as less important than achievement training, although its specific con-

tribution to the development of the motive was not entirely clear at the time the article was written. Little evidence of achievement training among Negroes could be found, although, regrettably, the data were skimpy. Since then, new data (which are scheduled for publication in the September issue of *Sociometry*) have reinforced my belief that independence training plays a limited role in the development of achievement motivation.

Dr. Rodman's [prediction] that the Negroes would tend to score high on Item 4 ("Nothing is worth the sacrifice of moving away from one's parents") proved to be correct. Fewer Negroes agreed with this statement than any of the other groups. Undoubtedly, this tended to "beef up" the Negro value score. His suggestion that many Negroes were reacting to white interviewers and the implied stereotype in their response to Item 7 ("Nowadays with world conditions the way they are, the wise person lives for today and lets tomorrow take care of itself") is more difficult to assess—biases of this type always are. The data show, however, that the Negro score for this item is almost identical with those of the French-Canadians and Italians, lower than those for Greeks and Jews, and higher than that of the white Protestants. This item clearly is not contributing an excessive weight to the total Negro score.

Whether the Negro's responses to these and other value items are a reflection of his realistic appraisal of vocational opportunities, a reaction to "white" interviewers and implied stereotypical content—as Dr. Rodman believes, or a measure of achievement orientation, or perhaps a combination of these and other factors, is a question which only further research can answer. Until then, the unexpectedness of the data on Negroes reveals, perhaps, less about the Negroes than about the sociologist's own preconceptions of this group.

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ON "CRUELTY, DIGNITY, AND DETERMINISM"

To the Editor:

Dr. Gwynn Nettler's article, "Cruelty Dignity, and Determinism," (*American Sociological Review*, 24 [June, 1959], pp. 375-384) is unusual and arresting in both method and conclusions; but on examination a number of criticisms are evident. Study of the article's structure indicates that we have at hand a very large *ad hominem* argument; for Dr. Nettler judges from the allegedly cruel attitudes of nearly a thousand peo-

ple that their beliefs are wrong, the idea of "free will" is a bogey, and the idea of "responsibility" is deadwood.

Now, rather than study Dr. Nettler's argument closer than this, let me list the several different questions which were not as distinct as they should have been: (1) Is the world determinate? (2) Is there free will? (3) Is the concept of responsibility outmoded? (4) Are people who believe in punishment *really* cruel (sadistic)? (5) Is punishment effective, and if so, when? (6) Even if punishment is effective, does that mean it should be used? (Compare torture.) If so, when? Although the last two questions bear on whether or not we should believe in punishment, Dr. Nettler did not discuss them; rather, he at once labelled as "cruel" anyone who believed in punishment (operational definition?) and "corroborated"—this from their attitude toward free will. While the article is on its empirical side a revealing study of the attitudes of an influential minority, in its use to support a philosophical thesis we see that the empirical method without analysis can bring confusion. Let me instantiate.

To believe in responsibility you need *not* be an indeterminist (though Nettler asserts differently). The indeterminist can hardly believe you responsible for a causeless act! The advocate of punishment, unless he is a complete ogre, must be partly a determinist: far enough to believe that punishment determines a man's future virtue. These are logical troubles in . . . Nettler's argument. . . .

Do people really have free will? I am quite sure that they do. But cause and quantum are not really relevant. Nobody is asserting magical causation from outside the head, and nobody is interpreting Heisenberg to make any *mental* event uncaused, since even a chance quantum jolt is a real cause on the neural level. Rather the argument for free will rests on philosophical analysis of the way the term is used. We must find whether or not some statements about free will have enough sense to be retained. It may be objected that the choice is phony, depending on how we choose to define the term. Not so. Admittedly it is "a question of definition;" but the question is whether the *everyday* definition of the word is a good one. We must see if there is an important core among the divergent ways the word is vulgarly used.

I will not bother to enumerate and cross off meanings. My case is this, simply put: the word "free will" is very useful to distinguish those cases in which people are free from physical constraint, drug-stupor, pain and dire threat, and may thus presumably make *free* decisions, from those cases in which they are not. Unless

we are to do away with the contract and the promise, to rely only on unbaked foggy trust all the time, the twin concepts of free will and responsibility have got to exist. We must know when to take a man at his word; when someone can be made to pay for damages; when formal conditions of trust and choice pertain. There are, it is true, degrees of free will, different categories of competence we are recognized as being in. There are degrees of responsibility, and cultures differ on when they ascribe it, but that makes it no more pernicious or "metaphysical" a notion. When a man is responsible for himself, nobody is responsible for him; when criminals act from "free will," we mean they are not hypnotized, or drunk, or feverish.

It is often said by earnest humanitarians that since all human acts are caused, the acts of the criminal are unfree because he was caused to make them. But this then erases the distinction we just made, useful all day long for everybody, between free will and unfree as describable in terms of their circumstances. To be sure, the boundaries of responsibility are somewhat arbitrary, but [the concept] still has great utility.

The most important problem for the humanitarian—here I am sure Dr. Nettler will agree—is not what is, but what should be. The question of whether an offender has free will, or a wart on his nose, is nowhere near as important as what can be done for rehabilitation of criminals and prevention of crime. I think the latter are entirely distinct from the philosophical issues.

THEODOR H. NELSON

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REPLY TO NELSON

To the Editor:

Mr. Nelson's shotgun assault upon "Cruelty, Dignity, and Determinism" hits no targets.

(1) *Argumentum ad hominem* has not been committed: The "argument to the man" is a fallacy because it is a type of *ignoratio elenchi*; it avoids the issue by reference to the characteristics of the men disputing it. My thesis concerns the relationship between the different assumptions we make about human behavior and their alleged and actual consequences. Concern with what a man believes does not avoid the issue here; it is the issue.

(2) I did not judge the attitudes of "nearly a thousand people" to be cruel.

(3) I did not judge the beliefs of "nearly a thousand people" to be wrong, nor did I make any statement about the beliefs of cruel people being "wrong" in an ethical sense (although I

am capable of doing so). I did suggest that indeterminists, who tend to be more cruel than determinists, maintain beliefs that are "wrong" in that they (a) run counter to other of their assumptions, (b) counter to the assumptions required, to date, by scientific endeavor, and (c) counter to the humanitarian objectives we all claim as our own.

(4) Nelson's questions #1 through #6 are mostly irrelevant to my thesis. Where they are not irrelevant, they are unanswerable because improperly asked (compare his questions #1 and #2 with the introductory paragraphs of my essay), or they have been met in the original paper (p. 375, n. 3; pp. 383-384).

(5) Yes, one may call "cruel" those people who believe in punishment in the context of "disinterested revenge" and who are unclear or ignorant of what hurting others will achieve. Whether they are "really cruel (sadistic)" is Nelson's question, and irrelevant.

(6) No one said that only indeterminists believe in "responsibility." Quite the opposite is maintained on page 383, where it is also argued that this word means more than one thing. Voiding the Canon of Singularity is still poor semantic sport.

(7) When Nelson attributes "free will" to actors who are "free from physical constraint, drug-stupor, pain and dire threat," he has uttered a definition. The fact that this definition recognizes only certain kinds of constraint—principally physical—as affecting behavior may be poor psychology but it remains his definition. What he can do with it in understanding behavior and, hence, in changing the unacceptable action of others, seems rather obvious and unfortunate.

(8) If the statement "... criminals act from 'free will'" means "... not hypnotized, or drunk, or feverish," why not merely say so? Why load the description of behavior with a terminology that carries an emotional freight and a baggage of assumption that diverts attention from our descriptive job?

(9) Contract and promise can be qualified by legal, moral, and interpersonal definition in numerous ways. It is probably true, as Nelson seems to suspect, that determinists are able to acknowledge a wider spectrum of contingencies affecting performance of promise than drugs, pain, and dire threat. And this ability may affect their response to behavioral deviation, as my paper shows.

(10) People can be "made to pay for damages" without recourse to Nelson's definition. "Made to pay" is a function of power. If Nelson is seeking through his definition to legitimize the differential application of legal power, there

are better modes of justification that do not quarrel with our growing knowledge of human behavior.

(11) Nelson's "free will" cannot tell us when to take a man at his word. Trust involves prophecy. Used alone, his definition will lead to erroneous prediction. If, acting upon his definition and its alleged consequences, Nelson trusts people possessed of his "free will" and coerced into deception only by drugs, pain, or dire threat, he must be an oft-disappointed man. Most of us live among people who, when they betray us, do so out of "nothing" more compelling than self-interest or a rather epidemic neurosis.

(12) The trouble with many humanitarians is

that their concern with what-should-be proceeds heedless of what-is and of what their assumptions about man do to him.

In his last paragraph, Nelson comes to the point of my paper: What people can conceive and are apt to prefer as responses to moral deviation are functions of their assumptions about human behavior. Therefore, "the rehabilitation of criminals and the prevention of crime" can *not* be divorced from our conceptions of the "philosophical issues" that we bring to our planning tables.

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THE PROFESSION: REPORTS AND OPINION

SOCIOLOGISTS AND SOCIOLOGICALLY TRAINED "PRACTITIONERS"

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Sociologists in this generation have a right to a rather optimistic view of their past and future. Not only have the social sciences increased in their relative importance in the general academic picture, but the public has become more aware of their existence. Without question there is a constant flow of social science information into the public domain and there appears to be a growing acceptance of the competence of social scientists. Commensurate with these developments, more persons train for or are diverted into fields of application related to these scientific disciplines.¹

As the dramatic example, since the World War II period, the increase of interest in the applied areas of psychology has been so great that more psychologists are now doing applied or clinical work than are engaged in the traditional academic and research tasks. This creates, it seems, a serious problem within the discipline when the interests in the applied areas are focused on *practice*, since these interests may not be compatible with the more traditional ones. A similar set of tendencies may be noted in the field of sociology, though to a lesser extent. For this reason it is well to anticipate some of the possible directions of change for the discipline in order to manage the scientific values involved to the best advantage. Sociologists can either take an active interest in this by giving it formal attention, or they can let the outcome be decided by default.

Generalizing findings for public consumption constitutes a continuing task for social scientists. On the one hand, this occupies persons who teach the social sciences at the "lower" level, where

essentially the task is communication of current achievement rather than the development of new ideas or the empirical verification of theoretical propositions. This communication function also occupies those persons who write articles about the social and psychological sciences for the more erudite magazines with wide circulation. On the other hand, this task involves persons who attempt to make social science information available for use in practical situations. Possible applications occur in medicine, social welfare, family relations, education, intergroup relations (including racial, ethnic, and religious relations, and the like), housing, and city planning.

It needs to be emphasized, however, that the person who participates in planned action, that is, converting a policy statement into material consequences, is performing an administrative function. As a social science consultant or "expert" he may bring to the attention of administrators information relevant to the topic, and on this information may be based the strategy of action. He has not done applied research, however, except in the sense of collecting and organizing information about what is known or supposed at the given time. This would be applied sociology in a meaningful sense only if the nature of the task involved something special in the background of the sociologist that makes him uniquely prepared for it rather than merely giving him a head start in doing the job. Newspapermen, administrators, practitioners in professions, and even undergraduates are proficient in getting certain kinds of information.

It is not facetious to maintain that what a sociologist does is not necessarily what should be identified as sociology. For example, the sociologist who deals successfully with the military is effective because he has developed an understanding of the norms and values involved, not because he is a sociologist. The representative of the large corporation must do the same kind of thing, and so must many others. More pointedly, the sociologist who gets along well with his wife is successful in this respect not because he is a sociologist; but married persons manage to get along and to do acceptable things. (Of course, it is possible to maintain that the sociologist may be socially effective precisely because of his sociological competence, but then we would be in the awkward position of having

¹ While most of the literature on the profession is dispersed in official reports and unofficial conversations, two recent discussions are worth reading or rereading in the context of "sociology as a profession": Donald Young, "Sociology and the Practicing Professions," *American Sociological Review*, 20 (December, 1955), pp. 641-648; and Talcott Parsons, "Some Problems Confronting Sociology as a Profession," *American Sociological Review*, 24 (August, 1959), pp. 547-559.

to explain all our failures: If you're so smart, why ain't you rich?)

The fact that the action planned and executed is in accord with the desires stated in the policy does not mean that it necessarily confirms the validity of a theory that the social scientist may have advanced in support of the action. In social action it is not a simple matter to confirm hypotheses generated by a theory. The best one can say is that the action seems to be in accord with the expectation derived from the theory. One cannot confirm hypotheses based on a theory in this kind of application unless one can establish that the consequences were necessary, and further that alternate theories would be inadequate to explain the phenomena. To establish that the action taken was necessary and sufficient to result in the observed consequences requires a rigorous kind of scientific endeavor, one that is practically never found in the study of social action. To the contrary, as Simon has pointed out, most action on the part of decision makers is at the level of sufficing rather than maximizing.² It may be that at a given time in terms of a particular social problem there are many possible courses of action, and any of these will lead to the intended consequences. But since the social action is usually unique, how can one establish this *post hoc*, or how can one establish that the proposed procedure was the most appropriate one, and that this choice is related to some known theory? The problem is particularly acute when one takes into account the human inclination for rationalization. Since a unique action cannot be performed again "the other way," there is perhaps an almost inevitable tendency to reinterpret the intended consequences or to see all past events as "for the good." The truth of a given theory needs to be demonstrated in scientific terms rather than in the practical sense. It is not sufficient, for example, for the medical practitioner to say that his therapeutic procedure was effective because the patient is well. Effectiveness of a therapeutic procedure must be determined in controlled experimentation or observation.

Similarly, as the medical practitioner is the consumer and less often the creator of scientific information, so the specialist in social action is a consumer of scientific information and rarely the creator. (Even a cursory examination of the relative rewards should dispel any tendency to interpret these comments as indicating that the scientist is more "important" than the practitioner.) On the other side of the coin, we would consider it inappropriate to train a person in

research in physiology, and then to grant him a license to practice medicine. What is occurring in psychology today, when persons who are presumably trained for research purposes are certified as practitioners, clearly involves an element of such inappropriateness. In the medical profession the usual course of training has been primarily for practice, not a preparation for research. Those physicians who go into research subsequent to their internship usually must first learn something about research methods, which may be accomplished by laboratory or field apprenticeship.

Graduate Departments versus Professional Schools: There are graduate schools with departments of sociology and there are also schools that are concerned primarily with the training of administrators, of specialists in human relations, intergroup relations, or other applied areas. Sociologists should examine the question of whether, in developing practitioners, it would be better to divert energy into bolstering the social science offered in these schools for practitioners or to attempt to design programs of graduate study that correspond to and compete directly with fields of application.

The question of whether practitioners should be trained in graduate schools or special-purpose schools of education, business administration, public administration, social welfare, and the like, is not a trivial one for the profession of sociology. Not only can use of graduate school resources for training practitioners be extremely wasteful, it can also lower the standards necessary for training research personnel. The claim that the graduate school can produce a higher caliber of practitioner has only a questionable degree of relevance. The argument that training practitioners in graduate schools may help to provide greater resources, more personnel, and larger enrollment makes sense only in institutions highly dependent on tuition for operation. This is a false hope, however, because the sources of money notoriously become the sources of power and policy.

Is graduate school training, particularly in research, necessary for the training of adequate practitioners? To my knowledge, there is no field in which it is expected that a practitioner will be trained in research; nor can the assumption be made that a person who is well trained in research will make a good practitioner. In fact, the attitudinal or personality requirements for effective practice, on the one hand, and capable research, on the other, may be quite antagonistic to each other. For example, the practitioner needs to have confidence in himself, otherwise he will not be able to inspire confidence in others; but the research worker often

² Herbert A. Simon, *Administrative Behavior*, New York: Macmillan, 1957.

needs to be self-questioning and tentative, otherwise he will prejudice the nature of his investigation. While there are analogies between a diagnostic procedure and a scientific investigation, the former deals with the single case, while research seeks to establish the necessary and sufficient conditions for the occurrence of given classes of phenomena.

It is not unusual for a person with a doctorate in sociology or some other behavioral science to be thrust into or gravitate toward a position of an administrative or practical nature. That this occurs does not mean that his training was necessary to reach such a position. The selective processes by which courses of study are chosen are notoriously capricious, and after undertaking an academic program the Ph.D. candidate may find it was a poor choice, or he may find that his values and needs have changed in the process of his training. On the other hand, it is naive to expect that because a person has gone through the long and arduous procedure of securing a Ph.D. he should sacrifice his other ex-

perience and talent. Thus it is quite possible that he may have administrative capacities, or may be better suited to be a clinical practitioner—or may be unique in his ability to keep his tasks straight and therefore to do effective research, good administrative work, and practice of a high caliber. The issue, I believe, is to properly identify the tasks and the talents.

It would seem that the wise investment of the social sciences in the graduate schools would be to develop scientists who can make their services available as scientists; practitioners should be trained in professional schools with social scientists participating where relevant. A social scientist may of course specialize in a substantive field, which may be defined either in terms of a whole segment of a systematic view of sociology or as an institutional area, such as religion, industry, or medicine. The job of the social scientist in these areas is to teach, to help uncover and chart empirically based social science theory—not to apply routine solutions in practice.

OFFICIAL REPORTS AND PROCEEDINGS

Notice Concerning the 1960 Annual Meeting

The 1960 Annual Meeting of the Society will be held at the Statler-Hilton Hotel, New York City, August 29, 30, and 31. The following sessions and chairmen have been arranged by President Howard Becker and the 1960 Program Committee:

Sessions and Chairmen

History of Social Thought up to Herbert Spencer
Rollin Chambliss, University of Georgia

History of Sociology from Spencer to Beginning of World War II

Charles Loomis, Michigan State University

History of Sociology During and After World War II

Roscoe C. Hinkle, Ohio State University

Current Sociological Theory—Current in the Senses Both of "Very Recent Date" and "Forefront of Attention"

Louis Schneider, Purdue University

Sociology in Its Relation with History

Robert K. Merton, Columbia University

Methodology

The Basic Assumptions

Frank E. Hartung, Wayne State University

Procedures of Sociology: Models and Their Uses

Herbert Hochberg, Northwestern University

Procedures of Sociology: Typology

Burkart Holzner, University of Wisconsin

Techniques of Sociology: Collection of Data

Irwin Sanders, Harvard School of Public Health

Techniques of Sociology: Validation

Clifford Kirkpatrick, Indiana University

Sociology of Knowledge, Including Sociology of Science

William L. Kolb, Carleton College

Sociology of Religion

Arnold Nash, University of North Carolina

Social Psychology

Basic Assumptions of Social Psychology

Sanford Dornbusch, University of Washington

Relations of Social Psychology with Psychology and with Sociology

John Clausen, National Institute of Mental Health

Social-Psychological Study of Perception, Communication, and Role Behavior

To be announced.

Symbolic Interaction

Ralph H. Turner, University of California, Los Angeles, Calif.

Small Group Analysis

A. Paul Hare, Harvard University

Sociology of Complex Organizations

Peter M. Blau, University of Chicago

Sociology of Power Relations

Morris Janowitz, University of Michigan

*Sociology of Ideological Conflict*Robert C. Sorensen, This Week Magazine, 485
Lexington Avenue, New York*Sociology of the Family*Nelson Foote, General Electric Company, New
York City*Social Structure and Personality*

J. E. Hulett, Jr., University of Illinois

Sociology of Education

Orville G. Brim, Jr., Russell Sage Foundation

Sociology of Locality Relations (Community,
Rural, Urban, etc.)

William H. Sewell, University of Wisconsin

Sociology of Law

R. A. H. Robson, University of British Columbia

*Social Differentiation and Stratification*Robert A. Nisbet, University of California,
Riverside*Race and Ethnic Relations*

George Lundberg, University of Washington

Demography

Norman B. Ryder, University of Wisconsin

*Sociology of the Arts*General Chairman: Theodore Caplow, Univer-
sity of Minnesota*Sociology of the Graphic and Plastic Arts*

M. Lee Taylor, University of Minnesota

Sociology of Literature

Milton Albrecht, University of Buffalo

*Sociology of Music*Paul Honigsheim, P. O. Box 325, East Lansing,
Michigan*Sociology of Popular Culture and Recreation*

Bernard Rosenberg, City College, New York

*Sociology of Work**Industrial Sociology*

Eugene Schneider, Bryn Mawr College

Sociology of Occupations and Professions

Raymond W. Mack, Northwestern University

Public Opinion and Mass Communication

John W. Albig, University of Illinois

Sociology of Planning for Development

Lyle W. Shannon, University of Wisconsin

Sociology of Deviation

Marshall B. Clinard, University of Wisconsin

Sociology of Medicine

Eliot Freidson, City College, New York

*Sociology and Mental Health*Harriet R. Mowrer, 4037 Fairway Drive, Wilm-
ette, Illinois*Social Change*

Alvin Boskoff, Emory University

Members may submit papers directly to the chairmen or, if they are uncertain about the appropriate chairman, to the Program Committee, in care of Michael Hakeem, 434 Sterling Hall, University of Wisconsin, Madison, Wisconsin. Papers should not exceed 1,500 words in length and must be received by February 1, 1960 at the latest. In order to allow the widest opportunity to participate in the annual program, each person is allowed to read only one paper if he is the sole author, and to contribute to two programs only if he is joint author in each case or is a chairman or discussant in one case.

**Auditor's Report for the Year Ended
November 30, 1958**

March 3, 1959

Council

The American Sociological Society
Washington Square
New York, New York

Gentlemen:

In accordance with instructions, we have examined the financial records of the American Sociological Society for the fiscal year ended November 30, 1958. We submit herewith the following Exhibits:

Statement of Cash Receipts
and Disbursements for the
Fiscal Year Ended
November 30, 1958

Exhibit A

Statement of Securities
Transactions for the
Fiscal Year Ended
November 30, 1958

Exhibit B

The accounting system of the Society is limited to a cash receipts and disbursements basis, only cash journals being used to record financial transactions.

The Cash Balances as of November 30, 1958, were confirmed directly to us by the depositories. We made a physical count on February 26, 1959,

of the stocks and bonds listed in Exhibit B. Verifications in connection with other assets and any liabilities of the Society as of November 30, 1958, has been omitted. The only cash receipts confirmed by reference to outside sources were dividends on stocks and bank interest income. We made tests to ascertain that membership dues, *Review* subscriptions and sales, *Review* advertising, and other types of receipts were properly entered in the cash receipts journal, and that all such receipts were properly deposited in the banks. In addition, we made an examination of the paid invoices and payroll and compared them with entries in the cash disbursements journal.

The book values shown for the securities on hand at November 30, 1958, which were purchased subsequent to November 30, 1948, are stated at cost, whereas the values shown for securities acquired prior to that date are stated at values obtained from previous Auditors' reports; adjustments being made thereto to reflect

capital changes. The November 30, 1958 market values represent the published redemption values for the bonds and the last closing Stock Exchange price prior to December 1, 1958, for the stock.

In our opinion, subject to the foregoing comments, the accompanying Statement of Cash Receipts and Disbursements (Exhibit A) and Statement of Security Transactions (Exhibit B) present fairly the cash transactions of The American Sociological Society for the fiscal year ended November 30, 1958.

We wish to express our appreciation of the courtesies extended to us by the Executive Officer and her assistants during the course of our examination.

Respectfully submitted,
KING AND COMPANY
Certified Public Accountants
68 William Street
New York 5, N. Y.

See next page for Auditor's report.

THE AMERICAN SOCIOLOGICAL SOCIETY
EXHIBIT A

STATEMENT OF CASH RECEIPTS
AND DISBURSEMENTS

FOR THE FISCAL YEAR
ENDED NOVEMBER 30, 1958

	Cash Receipts	Cash Disbursements	Excess (Deficiency) of Receipts Over Disbursements
<i>Membership Dues:</i>			
Fellow	\$ 9,303.00		
Active and Associate	35,275.74	\$ 106.50	
Student	9,217.25		
Joint	243.00		
Asian (Under Grant)	81.30		
Life	1,030.00(E)		
	<u>\$ 55,150.29(A)</u>	<u>\$ 106.50</u>	\$55,043.79
<i>American Sociological Review:</i>			
Subscriptions	\$ 14,955.49	\$ 106.70	
Asia Foundation Funds Allocated	375.00(D)		
Sale of Back Issues	2,999.47	7.25	
Advertising Income	6,449.40		
Printing and Mailing		38,652.60	
Clerical Salaries—Editor		3,713.10	
—Office		2,750.00(B)	
Editor's Expense		672.03	
Miscellaneous Expense		1,650.00(C)	
	<u>\$ 24,779.36</u>	<u>\$ 47,551.68</u>	(22,772.32)
<i>Sociometry:</i>			
Subscriptions	\$ 9,356.05	\$ 35.78	
Asia Foundation Funds Allocated	225.00(D)		
Sale of Back Issues	409.65		
Printing and Mailing		6,754.41	
Clerical Salaries—Editor		420.25	
—Office		1,800.00(B)	
Editor's Expense		241.35	
Miscellaneous Expense		900.00(C)	
	<u>\$ 9,990.70</u>	<u>\$ 10,151.79</u>	(161.09)
<i>Employment Bulletin:</i>			
Payments for Listings	\$ 182.55	\$ 3.00	
Clerical Salaries		1,200.00(B)	
Miscellaneous Expense		1,500.00(C)	
	<u>\$ 182.55</u>	<u>\$ 2,703.00</u>	(2,520.45)
<i>Index:</i>			
Sales	\$ 657.77	\$ 2.50	
Clerical Salaries		10.00(B)	
Miscellaneous Expense		10.00(C)	
	<u>\$ 657.77</u>	<u>\$ 22.50</u>	635.27
Carried Forward	\$ 90,760.67	\$ 60,535.47	\$30,225.20

THE AMERICAN SOCIOLOGICAL SOCIETY

EXHIBIT A—Continued

STATEMENT OF CASH RECEIPTS
AND DISBURSEMENTS (Continued)FOR THE FISCAL YEAR
ENDED NOVEMBER 30, 1958

	Cash Receipts	Cash Disbursements	Excess (Deficiency) of Receipts Over Disbursements
Brought Forward	\$ 90,760.67	\$ 60,535.47	\$30,225.20
<i>Directory:</i>			
Sales	\$ 153.75		
Clerical Salaries		\$ 10.00(B)	
Miscellaneous Expense		10.00(C)	
	\$ 153.75	\$ 20.00	133.75
<i>Research Census:</i>			
Report Sales	\$ 357.00	\$ 1.00	
Mailing Questionnaires		283.53	
Report Expenses		400.00(C)	
Clerical Salaries		700.00(B)	
	\$ 357.00	\$ 1,384.53	(1,027.53)
<i>Program Abstracts:</i>			
Sales	\$ 308.00	\$ 1.00	307.00
<i>Russell Sage Bulletins:</i>			
Sales	\$ 142.10		
Payments to Publisher		\$ 150.97	
Honoraria to Authors		250.00	
Clerical Salaries		100.00(B)	
Miscellaneous Expense		100.00(C)	
	\$ 142.10	\$ 600.97	(458.87)
<i>Current Sociology:</i>			
Subscriptions	\$ 193.10		
Payments to Publisher		\$ 166.28	
Clerical Salaries		30.00(B)	
	\$ 193.10	\$ 196.28	(3.18)
<i>Recruitment Brochure:</i>			
Distribution and Mailing Expense	\$	\$ 100.00(C)	
Editor's Expense		25.00	
Clerical Salaries		25.00(B)	
	\$ —	\$ 150.00	(150.00)
<i>Sociology Today:</i>			
Royalties	\$ 775.00		
Clerical Salaries		\$ 200.00(B)	
Miscellaneous Expense		50.00(C)	
	\$ 775.00	\$ 250.00	525.00
Carried Forward	\$ 92,689.62	\$ 63,138.25	\$29,551.37

THE AMERICAN SOCIOLOGICAL SOCIETY
EXHIBIT A—Continued

STATEMENT OF CASH RECEIPTS
AND DISBURSEMENTS (Continued)

FOR THE FISCAL YEAR
ENDED NOVEMBER 30, 1958

	Cash Receipts	Cash Disbursements	Excess (Deficiency) of Receipts Over Disbursements
Brought Forward	\$ 92,689.62	\$ 63,138.25	\$29,551.37
<i>Committees:</i>			
Executive (Committee and Council):			
Travel		\$ 945.95	
Clerical Salaries		1,000.00(B)	
Miscellaneous Expense		400.00(C)	
Nominations and Elections:			
Chairman's Expense		13.10	
Election Mailing		569.77	
Program—Clerical Salaries		1,500.00(B)	
—Miscellaneous Expense		300.00(C)	
Social Statistics—Expense		61.00	
Asia Foundation Travel:			
Available from Grant	\$ 375.00(D)		
Clerical Salaries		375.00(B)	
Carnegie Travel Grant:			
Available from Grant (To be Withdrawn from Savings)	290.00	290.00	
Committee Travel		139.69	
Miscellaneous Expense		150.00(C)	
Other Committees and Representatives:			
Travel		224.36	
Clerical Salaries		1,000.00(B)	
Miscellaneous Expense		250.00	
	\$ 665.00	\$ 7,218.87	(6,553.87)
<i>Social Psychology Section:</i>			
Fees	\$ 204.00		
Mailing Expense—Office		\$ 36.52	
	\$ 204.00	\$ 36.52	167.48
<i>Sections in Formation:</i>			
Clerical Salaries	\$	\$ 200.00(B)	
Miscellaneous Expense		200.00(C)	
	\$ —	\$ 400.00	(400.00)
Carried Forward	\$ 93,558.62	\$ 70,793.64	\$22,764.98

THE AMERICAN SOCIOLOGICAL SOCIETY
EXHIBIT A—Continued

STATEMENT OF CASH RECEIPTS
AND DISBURSEMENTS (Continued)FOR THE FISCAL YEAR
ENDED NOVEMBER 30, 1958

	Cash Receipts	Cash Disbursements	Excess (Deficiency) of Receipts Over Disbursements
Brought Forward	\$ 93,558.62	\$ 70,793.64	\$22,764.98
<i>Annual Meeting:</i>			
Meals and Receptions	\$ 1,332.60	\$ 1,274.32	
Committee Functions	122.50	282.39	
Abstracts—Clerical Salaries		300.00(B)	
—Mimeographing and Mailing		1,648.38	
Registration Fees	2,028.00		
Program—Printing and Mailing		3,618.47	
—Advertising Income	3,061.70		
Book Exhibits—Income	1,359.02	2.00	
—Expenses (Including Promotion)		404.82	
Miscellaneous Expense		564.30	
Clerical Salaries		200.00(B)	
Travel		726.78	
	\$ 7,903.82	\$ 9,021.46	(1,117.64)
<i>Office:</i>			
Executive Officers' Salary			
Part Time		\$ 4,729.09	
Clerical Salaries:			
Routine Member Mailings and Dues Collection		3,901.00(B)	
Correspondence		3,971.00(B)	
Filing and Miscellaneous		3,496.08(B)	
Printing, Mailing and Other			
Expenses—Membership Notices, Files, Etc.	\$ 9.48	3,979.38	
Office Maintenance	15.00	832.06	
Office Insurance	2.75	83.94	
Purchase of Office Equipment:			
Dictaphone		450.50	
Cabinet, Table, and Chairs		58.50	
Royal Typewriter		216.00	
Rent			
	\$ 27.23	\$ 21,717.55	(21,690.32)
<i>Other Journals for Members:</i>			
Subscriptions	\$ 10,406.70		
Payments to Publishers		\$ 8,765.55	
Clerical Salaries		1,000.00(B)	
Miscellaneous Expense		250.00(C)	
	\$ 10,406.70	\$ 10,015.55	391.15
Carried Forward	\$111,896.37	\$111,548.20	\$ 348.17

THE AMERICAN SOCIOLOGICAL SOCIETY
EXHIBIT A—Continued

STATEMENT OF CASH RECEIPTS
AND DISBURSEMENTS (Continued)

FOR THE FISCAL YEAR
ENDED NOVEMBER 30, 1958

	Cash Receipts	Cash Disbursements	Excess (Deficiency) of Receipts Over Disbursements
Brought Forward	\$111,896.37	\$111,548.20	\$ 348.17
<i>Robert MacIver Award Fund:</i>			
Savings Account Interest	\$ 102.59		
Royalties	104.13		
Award (Withdrawn from Savings)		\$ 500.00	
	\$ 206.72	\$ 500.00	(293.28)
<i>Carnegie Travel Grant Fund:</i>			
Savings Account Interest	\$ 275.08		
Travel of Delegates (Withdrawn from Savings)		\$ 375.00	
	\$ 275.08	\$ 375.00	(99.92)
<i>Miscellaneous:</i>			
Dividends (Exhibit B)	\$ 166.95		
Savings Account Interest—General	312.01		
Audit Fee		\$ 200.00	
Dues to A.C.L.S.		100.00	
A.C.L.S. Expenses (Balance Reimbursed in 1959)	109.78	176.13	
Mailing List Rental	1,783.57	1,190.31	
Security Bond		125.00	
Social Security Taxes—Net		220.38	
Legal Fees		237.82	
Miscellaneous Income and Expense	62.21	6.20	
Miscellaneous Reimbursed Expenses	82.00	68.00	
Insurance on Journals		178.10	
Travel Under Asia Foundation Grant	254.00(D)	254.00	
Grant from Asia Foundation—			
Allocable to Expenses of Future Years	1,200.00(D)		
Not Allocated	71.00(D)		
Purchase of Securities (Exhibit B)		136.19	
	\$ 4,041.52	\$ 2,892.13	1,149.39
TOTAL CASH RECEIPTS AND DISBURSEMENTS	\$116,419.69	\$115,315.33	
EXCESS OF CASH RECEIPTS OVER DISBURSEMENTS			\$ 1,104.36
CASH BALANCE—DECEMBER 1, 1957			47,197.90
CASH BALANCE—NOVEMBER 30, 1958 (Carried Forward)			\$48,302.26

THE AMERICAN SOCIOLOGICAL SOCIETY
EXHIBIT A—(Continued)

STATEMENT OF CASH RECEIPTS
AND DISBURSEMENTS (Continued)

FOR THE FISCAL YEAR
ENDED NOVEMBER 30, 1958

CASH BALANCE—NOVEMBER 30, 1958		<u>\$48,302.26</u>
Consisting of:		
General Funds:		
Checking Account—		
Chemical Corn Exchange Bank	\$25,611.35	
Disbursed for Carnegie Travel Grant Fund	<u>290.00</u>	\$25,901.35
Savings Accounts—		
American Irving Savings Bank		<u>10,685.64</u>
		<u>\$36,586.99</u>
Carnegie Travel Grant:		
First National City Bank—Savings Account	\$ 8,967.58	
Less: Due to General Fund	<u>290.00</u>	8,677.58
Robert MacIver Award Fund: First National City Bank—Savings Account		<u>3,037.69</u>
		<u>\$48,302.26</u>

Notes:

- (A) Includes membership dues for the calendar year 1959 of \$21,371.50.
- (B) Allocated portion of office salaries paid.
- (C) Includes allocated portion of office mailing expenses.
- (D) Allocation of funds granted by Asia Foundation.
- (E) Life membership receipts of \$1,030.00 plus \$200.00 of such receipts in the prior year were deposited in one of the General Funds savings bank accounts.

THE AMERICAN SOCIOLOGICAL SOCIETY
EXHIBIT B

STATEMENT OF
SECURITIES TRANSACTIONS

FOR THE FISCAL YEAR
ENDED NOVEMBER 30, 1958

	Balance November 30, 1957				Transactions during the Year				Balance November 30, 1958			Dividends Received During the Year
	Date Acquired	Face Amount or Number of Shares	Book Value		Purchases	Sales Proceeds	Profit	Face Amount or Number of Shares	Book Value	Redemption or Market Value		
Bonds:												
U. S. Savings	Series F due 6/1/57	1945	\$ 2,000.00	\$ 1,480.00	\$	\$	\$	\$ 2,000.00	\$ 1,480.00	\$ 2,000.00	\$	
U. S. Savings	Series F due 8/1/62	1950	8,000.00	5,920.00				8,000.00	5,920.00	6,992.00		
U. S. Savings	Series J due 3/1/66	1954	11,000.00	7,920.00				11,000.00	7,920.00	8,679.00		
U. S. Savings	Series J due 3/1/67	1955	11,100.00	7,992.00				11,100.00	7,992.00	8,524.80		
U. S. Savings	Series J due 6/1/68	1956	4,175.00	3,006.00				4,175.00	3,006.00	3,131.25		
Stocks:												
Standard Oil Company of New Jersey	Capital	1940-57	72	533.56	136.19 (A)			75	669.75	4,415.62	166.95	
				<u>\$26,851.56</u>	<u>\$136.19</u>	<u>\$</u>	<u>\$</u>		<u>\$26,987.75</u>	<u>\$3,742.67</u>	<u>\$166.95</u>	

Note: (A) In December 1957 the Society purchased 3 shares of Standard Oil of N. J. in exercise of 72 rights (one to 30 at \$44.00) received in the previous month and 18 additional rights purchased for \$4.19.

**Financial Report from the Executive Office,
May, 1959**

Table 1 summarizes the expenditures for the past year, comparing them with the authorized budget for that year, and indicating the extent to which the various activities of the Society were self-supporting (through subscriptions, advertising, and the like) or were supported from dues or special funds. This statement adjusts the cash figures as shown by the audit in order to fit the current year more exactly. Considerably increased expenditures, reflecting the rapid expansion of Society activity as well as the generally rising level of all costs, were not entirely offset by increases in income. Thus the year ended with a deficit close to \$4,000, as budgeted.

Table 2 shows the budget which has been au-

thorized by the Council for the fiscal year 1959. This budget provides for an increase in the number of pages as well as the circulation of the *Review*, publication of an enlarged Directory to be distributed free to all except Student members, wider distribution of the Employment Bulletin, analysis of data about members preparatory to publication of a Recruitment Brochure, expanding activities of Society Committees and Sections and of the Society's Executive Office, and so on. It is estimated that the cost of such services to members and to the profession can be met without a deficit this year, largely because of the change in the dues structure.

Respectfully submitted,

MATILDA WHITE RILEY
Executive Officer

TABLE 1. EXECUTIVE OFFICER'S FINANCIAL STATEMENT FOR THE FISCAL YEAR 1958

	BUDGET TOTAL	TOTAL ACTUAL	INCOME ALLOCATIONS		
			Dues	Special Funds	All Other (subsc., ads, etc.)
	(1)	(2)	(3)	(4)	(5)
EXPENDITURES					
I. PUBLICATIONS					
<i>Review</i>	\$40,671	\$41,239	\$16,802	\$375	\$24,062
<i>Sociometry</i>	9,444	8,802	(+1,153)	225	9,730
Employment Bulletin	1,800	2,700	2,520		180
Directory	20	20	(+134)		154
Index	20	20	(+635)		655
Research Listing	1,390	1,704	1,348		356
Program Abstracts			(+307)		307
Bulletins	1,135	601	(+91)	550	142
<i>Current Sociology</i>	255	196	3		193
Recruitment Brochure	1,000	150	150		
<i>Sociology Today</i>		250	(+525)		775
TOTAL	\$55,755	\$55,682	\$17,978	\$1,150	\$36,554
II. ANNUAL MEETING	\$ 4,673	\$ 5,537	\$ 1,251		\$ 4,286
III. OFFICE (excl. amt. allocated)	21,457	22,611	22,611		
IV. SECTIONS AND AFFILIATES	400	437	437		
V. COMMITTEES	5,953	7,079	6,414	665	
VI. MISCELLANEOUS	11,359	12,872	980	1,494	10,398
TOTAL EXPENDITURES	\$99,577	\$104,218	\$49,671	\$3,309	\$51,238
TOTAL INCOME	\$95,696	\$100,503	\$45,956	\$3,309	\$51,238
NET	\$(-3,881)	\$(-3,715)	\$(-3,715)		

TABLE 2. BUDGET FOR THE FISCAL YEAR 1959

	BUDGET AS AUTHORIZED	DETAILS OF PUBLICATION BUDGET	INCOME ALLOCATIONS		
			Dues	Special Funds	All Other (subsc., ads, etc.)
	(1)	(2)	(3)	(4)	(5)
EXPENDITURES					
I. PUBLICATIONS					
<i>Review</i>		\$46,748	\$21,136	\$ 750	\$24,862
<i>Sociometry</i>		10,100	(+320)	450	9,970
Employment Bulletin		3,500	3,300		200
Index		20	(+180)		200
Directory		4,200	3,700		500
Research Listing		1,900	1,350		550
Program Abstracts		—	(+307)		307
Russell Sage Bulletins		2,450	—	1,200	250
Recruitment Brochure		850	850		—
TOTAL	\$68,768		\$29,529	\$2,400	\$36,839
II. ANNUAL MEETING	\$ 5,600		\$(+572)		\$ 6,172
III. OFFICE (excl. amt. allocated)	29,718		29,718		
IV. SECTIONS AND AFFILIATES	2,156		1,676		480
V. COMMITTEES	10,190		9,515	\$ 675	
VI. MISCELLANEOUS	19,729		2,808	1,240	15,681
TOTAL EXPENDITURES	\$136,161		\$72,674	\$4,315	\$59,172
TOTAL INCOME	\$137,487		\$74,000	\$4,315	\$59,172
NET	\$ 1,326		\$ 1,326		

NEWS AND ANNOUNCEMENTS

FRANK DEKKER WATSON
1883-1959

Frank Dekker Watson came to Haverford College as Associate Professor of Social Work in 1914. In 1921 he became Professor of Sociology and Social Work, the chair he held until his retirement in 1948. At the time of his death, he was Emeritus Professor of Sociology.

Dr. Watson received his collegiate and professional training at the University of Pennsylvania, obtaining the B.S. degree in 1905 and the Ph.D. in 1911. His professional academic experience included teaching at the University of Pennsylvania, Temple University, Swarthmore College, and the New York School of Social Work. From 1917-1920 he was Director of the Pennsylvania School of Social Work in Philadelphia. His research and administrative activities comprehended such fields as employment, as a member of

the research staff of the famed U. S. Immigration Commission of 1911; participation and publication in labor legislation and arbitration; direction of the pioneering Swarthmore Race Relations Institutes of the 1930s; and, with his wife, Amey E. Watson, research and publication in family economics. His early publications include a study of that precursor of contemporary social work, the charity organization movement in the United States.

Orderly and calm of person, gentle and dignified of mien, Frank Watson was a pioneer in the development of curriculum offerings in the theory and application of the principles of social welfare and social reform. Upon his retirement he not only continued to serve Haverford College in several academically related capacities but also engaged in international social welfare programs in Greece.

Frank Watson's professional career encompassed a large portion of the academic era in which sociology, social work, and social reform were often regarded as one. Despite the theoretical and empirical changes in sociology that took place during this period, he served his students, the academic institutions, and the profession of sociology with a steadfast equanimity that enabled him to be ever attuned to that which is human in every person.

Haverford College

IRA DE A. REID

JOHN PEEBLES DEAN 1914-1959

John P. Dean died on June 9 in Cranston, Rhode Island, after a prolonged illness. He had been on leave of absence from his position as Associate Professor at Cornell University.

In John Dean's death the field of sociology has lost a distinguished scholar, teacher, and researcher. He received the B.A. degree from Dartmouth College in 1936, when he was elected to Phi Beta Kappa. After working in business for about a year, he began his graduate studies in sociology at Columbia University and received the M.A. in 1938. As was characteristic of John Dean, his everyday activities and experiences became part of his professional work: his observations of the world of business and finance were employed in writing his Master's essay, *A Study on the White Collar Clerk in Wall Street*. He was a Gilder Fellow in 1939-1940 at Columbia, where he completed the Ph.D. in 1944.

His doctoral research and dissertation were in the field of housing, a subject to which he brought keen analytical powers, always attempting to present his work in its larger social context. In addition to his sociological research in housing, he taught advanced courses in the subject and for a two-year period was on the staff of the Federal Public Housing Authority. Public and private organizations made valuable use of Dean's broad knowledge and experience in urban planning and housing design as well as in the economics and social aspects of housing.

John Dean's career as a college instructor began at Bucknell Junior College, Wilkes-

Barre, Pennsylvania. Subsequently he taught with distinction at Queens College, New York, the New School for Social Research, and Cornell University. Hundreds of students were stimulated by his rigorous and insightful instruction.

In 1948 he joined the Department of Sociology and Anthropology at Cornell as Field Director of the Studies of Intergroup Relations, directing a large staff and many graduate students in the application of a wide variety of field techniques. In a sense, Dean's metier was field work in intergroup relations for here he had the chance to use his creative powers on a problem of large human significance. Many of his ingenious approaches to field problems employing experiments, observation, and participant observation, as well as surveys, have not been published.

His numerous professional and popular articles, research reports, and chapters of books reflect the important facets of his career. He was the author or co-author of four volumes: *Home Ownership: Is it Sound?*, *The Book of Houses*, *Desegregation*, and *A Manual of Intergroup Relations*. The latter volume received the Ainsworth-Wolf Award in 1955. In addition to the honors cited above, Dean was a Rockefeller Fellow and a Post-Doctoral Research Fellow of the Social Science Research Council.

Throughout his career John Dean always tried to apply his interest in objective, thorough research on a subject of important human concern. He was a valued member of the National Association of Intergroup Relations Officials. While his health permitted, he was an able and willing participant in conferences, workshops, and professional meetings, always employing his knowledge and experience in the hope of making our society more democratic.

John Dean had a sensitive and inquiring mind and a humane spirit. His death represents a very great loss to social science and to the community at large.

GORDON F. STREIB

Cornell University

International Conference of Religious Sociology. The Sixth Annual Conference was held in Bologna, Italy, on September 3-6, on the theme of "Religion and Social Integration."

American Association on Emeriti. Members of the Society will be interested in the American Association on Emeriti, a relatively new organization which devotes itself to improving academic retirement systems and promoting the welfare of retired academic persons. Its membership includes both In-Service and retired professors and administrators, widows and widowers of former academic persons, and friends. It also provides for institutional, departmental, and donor memberships.

The Association takes a yearly Emeriti Census; publishes the *Emeriti for Employment*, through which it seeks to place such academic persons as are in good health and desire to continue to work; it sponsors a near-major medical plan for both pre-retired and retired persons; it plans to establish one or more Residence-Work Centers and a Supplementary Pension Fund. By these means it seeks to improve the terminal security of the academic profession and to enhance the power of higher education to recruit and retain capable personnel.

The Association has an office at 1025 Connecticut Avenue, NW, Washington 6, D. C. and maintains research center on the campus of the University of California, Los Angeles. Its president is Dr. Constantine Panunzio, Emeritus in Sociology, and for many years member of this Society.

The American Sociological Society, in cooperation with Section K, American Association for the Advancement of Science, announces the following meetings: (1) a symposium on "Family Structure: Process and Trends," with Hugh Carter, Clifford Kirkpatrick, Paul C. Glick, Evelyn Kitagawa, Norman Ryder, Robert F. Winch, and Irene B. Taeuber, Chairman, as participants; (2) a session on "The Use of Computers in the Simulation of Social Behavior," under the chairmanship of James S. Coleman. The meetings will be held at the LaSalle Hotel, Chicago, on December 29, 1959, at 9 A.M.

The Center for Research on Conflict Resolution has been established at the University of Michigan for promoting and carrying out research of relevance to problems of war and peace. Such research may focus on the resolution of conflict in other fields, such as industrial relations or inter-community relations, as well as international relations. It is hoped that the research approaches of sociology, anthropology, economics, and psychology can be joined to the established approach of political science, and that they may include projects of scholars at other institutions. The *Journal of Conflict Resolution*, now in its third year, has become one of the activities of the Center. The Chairman of the Center's Executive Committee is Robert C. Angell, to whom sociologists interested in participating in the program may write.

The Committee on Autonomous Groups. Joseph C. Bailey, Professor of Human Relations, Graduate School of Business Administration, Harvard University, has been elected Chairman, succeeding Walter W. Pettit, Director Emeritus of the New York School of Social Work, Columbia University, Chairman since the Committee was formed

in 1939. Glen Burch, Director of the Study-Discussion Program of the Ford Fund for Adult Education, has become Vice-Chairman. Maria Rogers continues as Secretary and succeeds Robert A. Nisbet, Dean of the College of Letters and Science, University of California, Riverside, as co-editor of *Autonomous Groups*, the Committee's quarterly publication.

The Educational Testing Service is offering for 1960-1961 its thirteenth series of research fellowships in psychometrics leading to the Ph.D. degree at Princeton University. Fellows will be engaged in part-time research in the general area of psychological measurement at the offices of the Educational Testing Service and will, in addition, carry a normal program of studies in the Graduate School. The closing date for completing applications is January 1, 1960. Information may be obtained from: Director of Psychometric Fellowship Program, Educational Testing Service, 20 Nassau Street, Princeton, New Jersey.

"Journal of Informal Sociology." Herbert J. Gans, Otto Pollak, and Marvin E. Wolfgang, of the University of Pennsylvania, and Eugene Schneider, of Bryn Mawr College, are planning to implement the proposal for a journal of "informal sociology," described in the August 1958 issue of the *Review*. This journal would publish presentations of provocative hypotheses; reconnaissance studies that may lead to fruitful generalizations; sociological analyses of current events; observations of phenomena and trends in American society and culture; commentaries and critiques on all phases of sociological studies. Articles and essays for possible publication in the journal are requested. It is anticipated that after the acceptance of about 20 papers local and national sources of funds for publication will be sought. Articles will be collected for three months following the publication of this notice, after which an attempt will be made to publish the journal—with the aid of editorial advisors from other universities. If plans fail, the articles will be returned promptly.

National Council on Family Relations. The E. A. Burgess Award (300 dollars) for the best research proposal for family research will be awarded in August, 1960. Research outlines, criteria, and other information may be obtained from Dr. Charles Bowerman, University of North Carolina, Chairman of The Award Committee.

Social Science Research Council. Fellowship and grant programs for the coming year embody some departures from previous policies and procedures. Prospective applicants are urged to consult the detailed announcement, which will be furnished on request, in order to avoid missing deadlines, some of which will be earlier than in past years. Closing dates for applications for some types of grants will be as early as November first. Preliminary inquiries should be addressed to 230 Park Avenue, New York 17, New York, at least three weeks in advance of the deadline.

Southwestern Sociological Society. Officers of the Society for 1959-1960 are: President, Warren Breed, Tulane University; Vice-President, Ethelyn Davis, Texas Woman's University; Secretary-Treasurer, Jack Dodson, University of Oklahoma. The Society will meet with the Southwestern Social Science Association in Dallas, April 15 and 16, 1960.

Albion College. Panos D. Bardis, Associate Professor of Sociology and Acting Chairman of the Department, has accepted an appointment with the University of Toledo and was recently appointed Editor of *Social Science*.

Bates College. Peter P. Jonitis is teaching in Ramallah under the sponsorship of the American Friends Board of Missions and the American Friends Service Committee during the current academic year.

Brown University. Course offerings in Anthropology have been expanded and the Department's name has been changed to Department of Sociology and Anthropology.

Kurt B. Mayer, Chairman, during the past summer lectured at the Hebrew University in Jerusalem and attended the International Population Conference in Vienna and the Fourth World Congress of Sociology in Stresa.

J. Louis Giddings has been promoted to Professor of Anthropology. He spent the summer in Alaska, continuing his archaeological explorations of beach ridges, under a grant from the National Science Foundation.

Vincent H. Whitney has resigned as Professor of Sociology to accept an appointment as Professor and Chairman at the University of Pennsylvania.

Basil G. Zimmer, formerly Resident Director of the University of Michigan Social Science Research Project in Flint, has joined the staff as an Associate Professor.

Robert O. Schulze has been appointed Assistant Dean of the college. He continues half-time in the Department.

Surinder K. Mehta has been promoted to Assistant Professor.

Dwight B. Heath has been appointed Instructor in Anthropology.

Kurt B. Mayer and Sidney Goldstein are continuing their two-year field study of problems of small business growth and survival. The Department of Sociology, together with the Department of Economics, has been awarded an additional grant by the Small Business Administration to undertake a study of "The Economic and Sociological Factors Influencing the Distribution of Business Firms by Size." The sociological part of this project will investigate the effects of suburbanization on the size and growth of business establishments. Surinder K. Mehta is directing the field work for this study.

University of California, Berkeley. Hanan Selvin participated in an S.S.R.C. Summer Seminar on Peer Group Culture and Adolescence at Ann Arbor, Michigan; and Reinhard Bendix, Herbert

Blumer, Kingsley Davis, Seymour M. Lipset, and Leo Lowenthal presented papers at the Fourth World Congress of Sociology in Milan-Stresa. Lipset also taught at the Salzburg Seminar in American Studies last summer.

Blumer, part-time Director of the Institute of Social Science, has returned to the Department from Brazil, where he served as a research consultant to the Center newly established there by UNESCO.

Davis, Lowenthal, and Selvin are on sabbatical leave for one semester during 1959-1960. Philip Selznick is serving as Vice-Chairman of the Department for 1950-1960, succeeding Kingsley Davis.

Neil Smelser is serving on the staff of the Center for the Integration of Social Science Theory. He and Erving Goffman, who has been promoted to Associate Professor, are the two sociology members of the Center.

William Petersen, formerly at the University of Colorado, has been appointed Associate Professor in the Department. He spent 1958-1959 in the Netherlands studying Dutch population patterns.

Robert Alford and Inge Powell have been appointed Lecturers for 1959-1960, and Ruth Kornhauser so appointed in the School of Nursing.

Rene König, Professor of Sociology at the University of Cologne, and Leonard Cain, Jr., Assistant Professor at Sacramento State College, are Visiting Professors during the current year.

William O. Nicholls, formerly Lecturer in Sociology at Columbia University, has been appointed Instructor.

Gertrude Jaeger Selznick and Ruth Ursula Gebhard have joined the staff of the Survey Research Center.

Carleton College. Frank C. Miller, Instructor of Sociology and Anthropology, has been awarded a pre-doctoral fellowship by the Social Science Research Council.

The City College. Robert Bierstedt, who taught at Stanford University during the Summer Session, is Fulbright Lecturer at the University of Edinburgh during the current academic year. Milton L. Barron has been elected Acting Chairman of the Department.

Adolph S. Tomars will continue his research in the sociology of music this year in Paris and London on a Guggenheim Fellowship.

Warren Brown has been appointed Director of the Social Research Laboratory.

Joseph B. Gittler is serving as Visiting Professor this year.

Anthony Leeds has been appointed Assistant Professor, and William M. Dobrin, Gerhard Hess, and Virginia Davis have been appointed Lecturers.

Melvin Herman, who teaches courses in social work, has been granted a year's leave of absence to serve as Executive Director of the New York Service for the Orthopedically Handicapped. Seymour Mirelowitz will teach Herman's courses.

Colorado State University. Winston W. Ehrmann, formerly of the University of Florida, has been appointed Professor of Sociology.

Cornell University. Edward F. Borgatta, of New York University and the Russell Sage Foundation, joined the Department in September.

Rose K. Goldsen has been promoted to Associate Professor.

Edward A. Suchman has resigned as Professor of Sociology, to serve as Director, Social Science Activities, City of New York Department of Health.

Allan R. Holmberg, Professor of Anthropology, was recently awarded a five-year grant by the Carnegie Corporation. The grant will be used for the study of Andean countries, for training of American students in Latin American anthropology, and to establish collaboration between Cornell scientists and their colleagues in numerous South American institutions. There are several fellowships available to graduate students in the program.

Gordon Streib is on sabbatical leave during 1959-1960 serving as a Fulbright Lecturer at the University of Copenhagen and the Danish National Institute of Social Research.

Norman Kaplan will spend the fall semester traveling in Europe working on his research project in the sociology of science.

William W. Lambert, has returned from sabbatical leave as Fulbright Lecturer at the University of Oslo, Norway.

William Delany and Wayne Thompson have received research grants from the Cornell Social Science Research Center. Delany is conducting an analysis of career orientations and group loyalties observable in a public bureaucracy and Thompson is conducting a study of local political behavior.

It is with deep regret that the Department reports the loss by death of John P. Dean. His obituary appears elsewhere in this issue.

Indiana University. Frank Westie has been elected by the Ohio Valley Sociological Society as its representative to the Council of the American Sociological Society.

John Liell received a Fellowship from the Inter-University Council on Social Gerontology to attend the Institute of Social Gerontology at the University of California in Berkeley last August. He also participated in an Inter-disciplinary Faculty Seminar on Metropolitan Problems at Syracuse sponsored by the Maxwell Graduate School of Syracuse and the Danforth Foundation.

Allen Grimshaw has been appointed Instructor in the Department.

An S.S.R.C. Post-doctoral Training Fellowship has been awarded to Sheldon Stryker for 1959-1960 for work at the University of Minnesota during his sabbatical leave.

University of Kansas. Carroll D. Clark, Chairman, was a member of the Chadron Nebraska State Teacher's College staff and Lawrence S. Bee served as Visiting Professor at Oregon State College last summer.

E. Jackson Baur has been promoted to Professor. He has received grants from the University General Research Fund to study the social adjustment of undergraduate students.

E. Gordon Erikson last summer worked in St.

Lucia, Barbados, and Granada of the Lesser Antilles, completing his field research on fertility expectations.

Carlyle S. Smith spent the summer in North and South Dakota in archaeological excavations of Northern Plains Indians.

Charles K. Warriner, now an Associate Professor in the Department, has returned from the Philippine Islands where he made a study of leadership among the Moros under a Fulbright Research Award.

Ray P. Cuzzort was granted an Elizabeth Watkins Research Grant for study during the past summer.

Robert Dentler has joined the staff as research associate in the Institute for Child Research in the School of Education; and Clarence Dale Johnson, formerly of St. Olaf College, has joined the staff as Instructor.

Everett C. Hughes, of the University of Chicago, was the Judge Nelson Timothy Stephens Lecturer in the School of Law, presenting three lectures on "Stress and Strain in Teaching and Learning." In March he also gave the Lindley Memorial lecture on "Quality and Equality: American Enterprises and Experiments in Education." He is the Rose Morgan Honorary Professor for the fall semester.

Kent State University. The name of the Department is now the Department of Sociology and Anthropology.

Michael M. Horowitz and John P. Burnell have joined the staff as Assistant Professor of Anthropology and Assistant Professor of Sociology and Anthropology, respectively.

Marvin R. Koller received a faculty fellowship to the Institute on Social Gerontology held at Berkeley last summer.

Lees Junior College. Erwin F. Karner, formerly at the State Teachers College, Dickinson, North Dakota, has been appointed to the faculty for the current year.

Lindenwood College. Helen P. Gouldner has been appointed Associate Professor and Chairman of the Department of Sociology. Walter M. Beattie, Jr., former Chairman, has resigned to take the position of Planning Director, Services to the Aging, Health and Welfare Council of Metropolitan St. Louis.

Los Angeles State College. The Department now offers a program leading to the M.A. in sociology.

The teaching staff includes the following: in sociology, Don J. Hager (Head) and Richard O. Nahrendorf, Professors; Karl M. Wallace, Harold T. Diehl, and Ester Penchef, Associate Professors; Gilbert Geis, Herman J. Loether, Paul K. Rowan, Robert Fulton, and Ralph Thomlinson, formerly of Denison University, Assistant Professors; in anthropology, Robert Ewald, Hal Eberhart, and Louis D. Faron, formerly of the University of Illinois, Assistant Professors.

Louisiana State University. Roland J. Pellegrin, Chairman of the Department of Sociology

and Head of the Department of Rural Sociology, has been promoted to the rank of Professor. Walfred J. Jokinen has been promoted to Associate Professor; in addition to his duties in the Department, he will continue to serve as Assistant Dean of the Graduate School.

John V. D. Saunders, formerly of Mississippi State University, has been appointed Assistant Professor of Sociology and Rural Sociology. In 1958-1959, as a recipient of a Fulbright Award, he spent nine months in South America, where he lectured at the University of Guayaquil and at the Central University of Quito.

John D. Kelley was appointed to an instructorship last February.

Rudolf Heberle, Boyd Professor, was elected President of the American Studies Association of the Lower Mississippi.

Alvin L. Bertrand was elected a Vice-President of the Southern Sociological Society, and has been appointed Chairman of the Society's Committee on the Profession.

George A. Hillery, Jr., Visiting Assistant Professor, has been appointed a post-doctoral fellow at the University of Florida for the 1959-1960 academic year.

McMaster University. Peter C. Pineo has joined the Department as Lecturer in Sociology. Frank E. Jones has been promoted to Associate Professor. Frank G. Vallee spent the summer months at Baker Lake, Keewatin District, Northwest Territories, carrying out a field study of the Eskimo.

Northwestern University. Raymond W. Mack has been appointed Chairman of the Department of Sociology to succeed Kimball Young, who has served as Chairman since 1947. Young will continue with his teaching duties as Professor of Sociology and with his research on communitarian social movements.

Gresham M. Sykes, formerly of Princeton University, will return from a year's leave in Mexico to join the Department as Associate Professor.

Scott Greer has accepted a joint appointment in the program of graduate studies in political behavior; he is Associate Professor of Political Science and Sociology. He is also Chairman of the Center for Metropolitan Studies, and will continue, within the framework of its program, his research on the social-political structure of metropolitan areas.

John Kitsuse and Aaron Cicourel have received a grant from the Graduate School to support their research on the organizational production of adolescent careers. Cicourel and Norton Long are engaged in a project dealing with the recruitment and assimilation of new personnel into the corporate structure.

Harold Guetzkow has been appointed Master-Fellow by the Ford Foundation for the five years, 1959-1964.

University of Oregon. Harry Alpert has been elected a Fellow of the American Statistical Association and is serving as a member of the Advisory Panel for Sociological and Economic Sciences, Na-

tional Science Foundation. He was awarded an Israel Summer Travel Fellowship for 1959 by an American faculty committee for the Hebrew University of Jerusalem. Alpert is Director of the newly created Office for Scientific and Scholarly Research which will coordinate research activities on this campus.

Joel V. Berreman continues to serve on a half-time basis as research associate with the Oregon Study of the Rehabilitation of Mental Hospital Patients.

Herbert Bisno returned to the campus in September with a promotion to Associate Professor. He has been on leave for over two years, first with the Curriculum Study of the Council for Social Work Education and during the past year as consultant to the Paul Baerwald School of Social Work, Hebrew University of Jerusalem.

Robert Dubin has received a grant from the Ford Foundation for support of his own research and fellowships for doctoral dissertations dealing with business problems. He has appointed Samuel Johnson as the first fellow. Dubin will be Ford Visiting Professor of Behavioral Sciences, School of Commerce, University of Wisconsin, for the 1959-1960 academic year. Patrick J. McGillivray has been appointed Instructor during Dubin's absence.

John M. Foskett is acting Deputy Director of the Institute for Community Studies. He is conducting in a new community a replication of studies made in Valley City I and Valley City II.

Walter T. Martin has been promoted to the rank of Professor. He was awarded a travel grant by the National Science Foundation to attend the World Population Congress and the meeting of the International Sociological Association.

G. Benton Johnson, Jr., and Lionel S. Wishneff have received a grant from the Office of Scientific and Scholarly Research to study values and social mobility. Wishneff has also been named Director of a three-year project to study resistance to educational television, financed by a grant from the United States Department of Health, Education, and Welfare under the National Defense Education Act. Staff members of three other departments are also participating. Miriam Johnson has been appointed Assistant Professor.

Theodore B. Johannis, Jr., has received a grant from the Office of Scientific and Scholarly Research for a study of "Patterns of Decision Making in the Family." He has been appointed Associate Editor of *Marriage and Family Living* and reappointed co-editor of the *Family Life Coordinator*.

James Price received a grant from the Foundation for Research on Human Behavior to enable him to continue his research on stability mechanisms of formal organizations.

The Department of Sociology received four fellowships under the National Defense Education Act. An intensive, accelerated program leading to research and teaching careers at the university level has been developed for the Fellows.

Purdue University. Two new staff members have been added: Robert K. Bain, formerly of the

United States Office of Education, and Sidney M. Greenfield, formerly of Connecticut College for Women, have been appointed Assistant Professors.

Louis Schneider is on leave of absence during the current year and is serving as Visiting Professor at Dartmouth College. Schneider is the newly elected President of the Ohio Valley Sociological Society.

Gerald R. Leslie was Visiting Professor during the first summer session of 1959 at the University of California, Berkeley.

Edward Z. Dager was Visiting Professor in Sociology during the second summer session at Kent State University.

Janice Partridge has left to accept a full time position in the Sociology Department at Wittenberg College in Springfield, Ohio.

Leonard Z. Breen has started a study of pre-retirement education among labor unions in the United States sponsored by the National Institute of Labor Education. This past summer Breen was recipient of a faculty fellowship in Social Gerontology, enabling him to attend the summer institute at the University of California.

The Purdue Research Foundation awarded summer research grants to James Beshers for studying statistical methods in urban demography, and to Walter Hirsch for studying autonomy of science in modern totalitarian systems.

Harold T. Christensen presented a paper, "Cultural Relativism and Pre-Marital Sex Norms," at the meeting of the International Sociological Association in September.

The local chapter of Alpha Kappa Delta has sponsored several visiting lecturers throughout the year, including: Albert K. Cohen, Indiana University; Reuben Hill, University of Minnesota; Francis Hsu, Northwestern University; Howard Perlmutter, Menninger Foundation; Dietrich Rietzel, George Williams College; William Seagle, U. S. Department of the Interior; Ernest Van Den Haag, New York University and New School for Social Research; and Kurt Wolff, Ohio State University.

University of Rhode Island. Department members are Professor L. Guy Brown, Associate Professor and Acting Chairman Irving A. Spaulding, and Assistant Professors Robert V. Gardner, Erwin H. Johnson, and Arthur H. Richardson. Johnson, along with Timothy O'Leary of Yale University and Thomas Hazard of Columbia University, is participating in an archeological surface and site survey of Indian settlements in southern Rhode Island. Richardson attended the Summer Training Institute in Social Gerontology at the University of California, Berkeley. A chapter of Alpha Kappa Delta will be activated at the University during the 1959-1960 academic year.

The St. Lawrence University. Donald Auster, formerly of the University of Indiana, and Roland B. Westerlund, formerly of the University of Illinois, have joined the staff.

Paul R. Ducey, Assistant Professor of Anthropology, has been reappointed Consultant in Archaeology to the New York State Education Department.

Donald J. Newman has been promoted to the rank of Associate Professor of Sociology.

Myles W. Rodehaver, Chairman, was elected President of the Upstate New York Sociological Society.

San Francisco State College. John W. Kinch joined the Department in September. Other members are Carlo L. Lastrucci, George E. Outland, Frederic W. Terrien, Donald L. Garrity, Don C. Gibbons, and F. D. Freeman.

University of Saskatchewan, Saskatoon. The College of Medicine has appointed Robin F. Badgley Assistant Professor in the Department of Social and Preventive Medicine, to fill the post of medical sociologist on the faculty. The post is the only one of its kind in Canada and is made possible by a grant from the Commonwealth Fund, New York.

Southern Illinois University. Joseph K. Johnson has retired as Chairman of the Department, but will continue as a member of the teaching staff. Paul J. Campisi, formerly of Washington University, has joined the staff and will serve as Chairman.

Eloise C. Snyder was awarded a fellowship to the Inter-University Training Institute in Social Gerontology conducted in Berkeley, California last August.

Western Reserve University. Irving Rosow, Director of the Ford Foundation Aging Project at the University, was awarded an NIMH Fellowship to the Summer Institute in Social Gerontology at Berkeley.

Marvin B. Sussman has been appointed Program Chairman of the Groves Conference on Marriage and the Family to be held in Columbus, Ohio, in April, 1960. He also received a substantial grant from the Association for the Aid of Crippled Children to finance a conference to be held in June, 1960, which will be limited to 20 of the major investigators doing research in this field.

Ruth Neuer, of the University of Pennsylvania, has joined the staff as Visiting Lecturer in Sociology for the academic year.

E. Gartely Jaco has joined the Department as Visiting Associate Professor of Sociology and Research Associate in Psychiatry. He is Director of Social Science Research at the Cleveland Psychiatric Institute.

University of Wichita. Stuart A. Queen is Visiting Professor again this year.

Lowell D. Holmes joined the faculty in September as Assistant Professor. He is from Missouri Valley College, Marshall, Missouri.

Donald O. Cowgill was a Fellow at the Institute on Social Gerontology at Berkeley, California in August.

REVIEW ARTICLE

THE WHOLE STATE OF SOCIOLOGY *

WILBERT E. MOORE

Princeton University

It is perhaps meaningless to say that sociology has "come of age," because the life cycle so implied is by no means clear. It is safer to say that sociology has grown, by any conventional test, that it has gained increasing if grudging recognition outside its own professional ranks, and that its practitioners encourage one another in virtue while maintaining an attitude of tentative bravery against the enemy without.

Four recent collections of essays lend credence to the impression of progress in sociological scholarship. Were it otherwise, our more morose moments would be validated and our occasional sense of impending doom justified.

Sociology Today presents revisions of papers originally prepared for the 1957 annual meeting of the American Sociological Society, under the presidency of Robert K. Merton. (An earlier "review of the field" when Louis Wirth was president of the Society in 1947 received partial publication in this *Review*, but did not become a book.) With fair consistency the authors have presented an appraisal of its current state with reference to the more or less standard specialties encompassed by contemporary sociology, and

with somewhat less consistency the authors have essayed the task of identifying current problems of theory and method.

The *Symposium on Sociological Theory* (hereafter *Symposium*) consists of nineteen longish chapters and an extended introduction by the editor. "Theory" here means primarily a concern with assumptions, concepts and conceptual orientations, logic, "methodology" in the sense illustrated by discussions of constructed types and models, and epistemology (not elsewhere classified). Many of these concerns would have been known to an older generation of do-it-yourself sociologists as "philosophical." As a tardy member of that generation, I confess to holding that reactionary view. The *Symposium* has rather little to tell us about the predictable character of social interaction, social structure, or orderly transformations of social orders. The editor's third introductory sentence reads: "Sober is the thought that if we could say nothing in sociology but what could be proved conclusively there would be no sociology." Professor Gross, meet *Sociology Today*, and be of good cheer.

Review of Sociology (hereafter *Review*) and *Sociology in the United States of America* (hereafter UNESCO) are comparable in topical coverage to *Sociology Today*. Two of the authors in *Review* and five of the authors in UNESCO play the same positions in *Sociology Today*. I discovered no triple-threat men. There is one interesting example, however, of psychological regression. For UNESCO, John A. Clausen wrote on the "sociology of mental health," but for *Sociology Today* he wrote on the "sociology of mental illness."

What can one say briefly about so rich a store of learning? Clearly, not all that deserves to be said. I have rather singled out a few problems of some significance for an appraisal of the state we are in.

STAND FAST IN ONE SPIRIT

Within the memory of living men, sociologists learned much of the intellectual content of their

* *Sociology Today: Problems and Prospects*. Edited by Robert K. Merton, Leonard Broom, and Leonard S. Cottrell, Jr. Under the auspices of the American Sociological Society. New York: Basic Books, 1959. xxxiv, 623 pp. \$7.50. *Symposium on Sociological Theory*. Edited by Llewellyn Gross. Evanston, Ill.: Row, Peterson & Co., 1959. ix, 642 pp. \$7.25.

Two other books, although previously reviewed in this journal, should also be noted: *Review of Sociology: Analysis of a Decade*. Edited by Joseph P. Gittler. New York: John Wiley & Sons, 1957. ix, 588 pp. \$10.50. *Sociology in the United States of America: A Trend Report*. Edited by Hans L. Zetterberg. Paris: UNESCO, 1956. 156 pp. \$2.50.

The title and section headings of this essay derive from an idiom older than the conventional language of our trade. Since a number of sociologists are now joining some of the more intelligible literary critics in decrying the character of sociological prose, I hope that the somewhat playful use of religious rhetoric will not be taken as unduly offensive to good manners.

subject in terms of "schools" of theory. One may in fact still encounter an alien colleague who knows sociologists by sight but sociology only at a certain historic distance, and who taunts the faithful with their contention. The taunts are undeserved, for one remarkable feature of the contemporary state of sociology is its overwhelming adherence to one doctrine. That doctrine is the "structural-functional" approach.

It may be true, as my colleague Marion Levy maintains, that most sociologists have been talking prose all along, like Molière's middle-class gentleman. But, like that upstart gentleman, it is also true that they have learned to name their language only recently. For the last two decades sociologists have increasingly "talked the same language," but also reflected a growing similarity of stance and posture in confronting social phenomena.

I do not mean to minimize the preference of some sociologists for naming things, of others for asking "significant" questions, and of others for finding "significant" answers. But it does appear to me that the names, the questions, and the answers are largely concerned with the concept of social systems, and with their analytical parts and their interdependencies. Even our disputes are "family fights," which of course may be acrimonious, rather than conflicts between competing sovereignties. "Functionalism" is the explicit orientation of most of the substantive papers in *Sociology Today*, and is implicit in others.

Such uniformity could not be expected to last, marred only by an unreconstructed but guilt-ridden minority who rejected what they feared. If the ending of ancient, foolish debates is a mark of maturity, the ending of all debate would surely be a symptom of senility.

Several criticisms, or critical refinements, of functionalism are presented in the essays before us. Carl G. Hempel (*Symposium*, Chapter 9) approaches the assumptions and propositions of functionalism from the standpoint of the logician. He notes particularly the weak or non-existent validation of the assumption of self-regulation in functional systems. Alvin W. Gouldner, in the same volume (Chapter 8), questions the easy assumption of symmetrical interdependence and calls for an analysis of the variable autonomy of sub-systems. Seymour Martin Lipset in *Sociology Today* (Chapter 3) does not make a frontal attack on received doctrine, but does raise problems concerning the containment of conflict in political systems and notes that political sociology has been spared some of the extreme order-and-equilibrium notions prevalent in other fields of inquiry. Talcott

Parsons in his essay on general theory (*Sociology Today*, Chapter 1) constructs a "scheme of four levels of structural organization" and a "paradigm of the 'double interchange' . . ." From the latter he derives a proposition concerning dichotomization of the population and applies this to the political, economic, and educational fields. What is of interest here is that Parsons has loosened many of the conventional notions of functional integration, but he emerges with the refinement of "dynamic equilibrating processes," that is, with self-regulation again.

In a paper of major importance, to my mind, Reinhard Bendix and Bennett Berger (*Symposium*, Chapter 3) have suggested renewed attention to the many "dual tendencies," simultaneously present, which sociologists conventionally recognize but conveniently forget in their preoccupation with self-regulating systems. The authors use their extended exposition to draw the methodological moral that concepts and definitions embody "each theorist's image of society and . . . this image is the source of each theory's utility and unique blindness." I have independently used the same perception of an inherent dialectic as one of several vantage points for the systematic analysis of social change,¹ and I naturally feel that this adds value to the Bendix-Berger position.

FAITH IS THE EVIDENCE OF THINGS NOT SEEN

Sociology is now emerging from an era in which a "theorist" was the last man a newly created academic department would hire, and there was little felt urgency about getting to the last man. A theorist is still not the first man on such a list, as in economics, but his relative position has clearly improved. This despite the fact that we have no uniform specifications of theoretical qualifications, a fact amply illustrated by the theorists who have written essays in our four books.

Much of what passes for theory, as noted with specific reference to the Gross *Symposium*, consists of conceptual schemes, methodology, and constructed analytical models or typologies. Now this is not worthless or irrelevant labor, and a number of fine minds have been devoted to such work. It is, however, labor that is preliminary or conditional to theory in the propositional sense, and in this same sense ultimately accountable to the crude confrontation with observation and data. In this propositional sense, the papers that appear to be topical and "descriptive" contain more theory than those ad-

¹ In a paper, "A Reconsideration of the Theory of Social Change," to be published in a volume edited by Edward A. Tiryakian.

vertised as theoretical. The latter may be more significant in the future, but a betting man would be better advised to rely on established relationships among variables than on ways of thinking about variables that might some time be reduced to observation.

The view I am embracing has been admirably expressed by Robert Bierstedt (*Symposium*, pp. 141-142), who has a far more profound "philosophical" orientation than most of his colleagues:

A schema is sterile unless it can produce a theory. Substantive theory, in short, is propositional. Its propositions are assertions about society. Its concepts have referents in the empirical world. Its definitions are convertible *simpliciter*. Its conclusions have truth claims. And it is the ultimate goal of sociological inquiry.

I do feel that the practitioners of conceptual refinement get carried away into distinctions without a difference, or at least without a difference that probable techniques of observation and analysis will confirm. The closest analogy that comes to mind is the time Frank Notestein caught me carrying to three decimal places some arithmetic manipulations on quantities not conceivably accurate to the nearest thousand.

THROUGH A GLASS DARKLY

All is not black, however. For some of what is called methodology I think the preferable term is technology, meaning the term as a compliment. Paul Lazarsfeld's essay (*Sociology Today*, Chapter 2) brilliantly demonstrates the conjunction of significant questions and the technology of observation and analysis. We do, of course, still have a depressing number of published reports, happily not in these volumes, that justify trivial questions and garbled answers on the basis that research tools have been sharpened. Unless we have a powerful will to believe in the all-purpose tool kit, we are surely justified in a querulous, "For what?"

Technologists too can be carried away. Refinements then depend upon custom-made precision in observation, which in turn tends to limit radically the scope and location of problems. For wider generalization and particularly for sweeping "comparative analysis," one is likely to have to settle for some crudities in concepts and data, and therefore to use analytical techniques that are elementary or of only middling difficulty. It is encouraging to note some examples of such an interpretation of the "middle range."²

² Stanley H. Udy, Jr., in *Organization of Work: A Comparative Analysis of Production among Non-industrial Peoples*, New Haven: HRAF Press, 1959,

TONGUES OF MEN AND OF ANGELS

It is not surprising that inventors of new concepts and conceptual structures do not communicate well with lay audiences. C. Wright Mills to the contrary (*Symposium*, Chapter 1), there is no reason why they should. If Mills seeks a wider audience than his professional peers, there is no evidence that the latter have made him either their spokesman or their adviser on communicative strategies. It remains true, however, that we do seem to exhibit a distressing amount of bad syntax, contorted construction, and unnecessarily pretentious jargon. Charles H. Page notes this in passing in his analysis of our teaching (*Sociology Today*, Chapter 25), and as an experienced (and scarred) book and journal editor he exhibits commendable restraint.

It is surely unnecessary to argue here the uses and abuses of technical language. As I share some of Mills' views about empty technical or verbal preciosity, I feel regretful about the necessity of pointing out that a popular style neither assures correct conclusions nor, indeed, prevents substantial self-contradiction, tendentious exposition, and general confusion. It is surely a misuse of "sociological imagination"³ to create a mythical American social order and claim it be real.

FAITH WITHOUT WORKS IS DEAD

What, then, is sociology about? Presumably it is about patterned social relations, their consequences and interrelations, and their changes. Such a broad characterization leads to many special sociologies, where sociology is preceded by an adjective (such as "political") or followed by a prepositional phrase (such as "of law"). Many of these special sociologies are represented in *Sociology Today*, a few additional ones in *Review* (industrial, social institutions, and voluntary associations), and a few others in UNESCO (military, the professions). We lack only a sociology of sociology, as Page points out in the cited chapter, and he makes a good case for that being less self-devouring than it appears.

The broad characterization also leads to the problem of sociological imperialism, which Page discusses with reference to faculty relations. I must agree with him that our sensible current

has combined clear, empirically relevant concepts, descriptive data from the Human Relations Area Files, and relatively simple statistical tests of significance. He emerges with orderly propositions. The book is a gem, elegant but unpretentious.

³ A term Mills discusses briefly in *Symposium*, Chapter 1, and elaborates in *The Sociological Imagination*, New York: Oxford University Press, 1959.

tactics call for restraint on imperialistic claims, particularly in view of our limited capacity for successful exercise of hegemony over neighboring fields. We can only barely control what we have. I do personally confess, however, to a deep faith in sociology as *the* generalizing social science, and to the conviction that we shall eventually inherit the Kingdom of Earth, if not of Heaven.

Speaking of "works," it is unfortunate that industrial sociology, or, as expressed in the Foreword to *Sociology Today*, the "sociology of economic institutions," does not appear in that volume. The gap is partly redressed by Alvin W. Gouldner's chapter (18) on "Organizational Analysis" and Everett C. Hughes' chapter (20) on "The Study of Occupations." Industrial sociology does appear in *Review* (Chapter 10, by William Foote Whyte and Frank B. Miller) and in UNESCO (by Louis Kriesberg). Industry-society or economy-society relations, in either functional or dynamic perspective, are notably missing from these surveys. The authors tend to stop the subject at the outbound factory gates or, at most, in the environing community. Since "plant sociology" has a perilous proximity to managerial manipulation, it is well to remember that sociologists are also looking at bigger pictures.

FOR THE TRUMPET SHALL SOUND . . . AND WE
SHALL BE CHANGED

Some missing topics are regretfully recorded in the Foreword to *Sociology Today*. Not even this small act of penance is accorded the whole subject of social change. Kingsley Davis in his chapter (14) on demography is appropriately concerned with population dynamics, but also notes the desirability of other studies of trends and interplay in historic process. C. Arnold Anderson's discussion of "Trends in Rural Sociology" (Chapter 16) barely mentions the vast secular changes in rural life during the period under review—changes that surely help to account for the current crisis in scholarship in that field. Hornell Hart presents (*Symposium*, Chapter 7) a provocative attempt at synthesis of the evidence on the cumulative, exponential, or "loglog" rates of change, and on what he calls

"logistic surges." Although Hart implies comprehensiveness in his treatment by his silence on other modes and rates of change, we are not obliged to take that implication seriously.

If functional theory is not necessarily static, the fact is that, in general, it has been so. In Parsons' proposition discussed above, his dichotomization and equilibrating processes operate under the stated *condition* of "a relatively continuous process of change in the direction of increasing differentiation." Would it not have been fun to make the condition a variable and account for that?

I have come to the personal conviction that for most purposes the equilibrium model of social systems must be abandoned, as leading to too much distortion, particularly in treating change as external, accidental, and in any event regrettable.

On the other hand, I do not believe that social change will prove to be a useful topic in the long run, for dynamics can be incorporated into the convenient and conventional ways of analyzing structures. We need only permit our specifications of system to include uncertainty, tension, strain, drift, deliberate intervention, and yes, even evolution.

I cannot resist the temptation to retain the apostolic metaphor for one final query: Where shall we spend eternity? The question is clearly meaningless, sociologically. But where and how will we spend the next few decades, or the years remaining to us by grace of actuaries? Prediction in sociology has become a pathetically timeless matter. (It has not become, thankfully, a pathetically unreal matter, as in formal economics.) No science can be expected to predict unique events, but only the general components of unique events. But where are the forecasters? Must we have amnesia about the past, but also (and therefore) no capacity to see the future? Are we so frightened of the possibility of accidental extermination that we must avoid any reference to the shape of things to come, lest we be cursed for our courage? I have the abiding conviction that sociologists have some gifts of prophecy, but first they must see change as the natural order of life and regain their historical sense, and mission.

BOOK REVIEWS

American Voting Behavior. Edited by EUGENE BURDICK and ARTHUR J. BRODBECK. Glencoe, Ill.: Free Press, 1959. iv, 475 pp. \$7.50.

Voting behavior is a popular subject for inquiry in the social sciences these days, and interdisciplinary collaboration is a fashionable style of research. For sociologists, political scientists, psychologists, psychiatrists—even a random economist—the ballot box contains both a statistical universe large enough to be respectable and manifestations of central problems in their disciplines. In a period where the available data rarely match our burning hypotheses and each brand of learning openly confesses its own provinciality, any effort to bring a united front of methodology to bear on a field bursting with stubborn facts has an almost irresistible appeal.

American Voting Behavior is such a venture, launched according to the editors as "an experiment in interdisciplinary thinking," and devoted to the question, "Where do we go from here toward a better understanding of the voting process?" Eugene Burdick, a political scientist, and Arthur J. Brodbeck, a psychologist, have assembled an impressive group of twenty-four other social scientists and set them loose in search of answers which will link together their professional concerns. They have, moreover, taken as their text the four pioneering studies in the field—*The People's Choice*, *Voting*, *The Voter Decides*, and *Straight Fight*—and asked their contributors to develop their observations from this common line of departure.

The experiment does not quite come off. Among the twenty-two chapters, there are a number of substantial and/or provocative articles. As an auspicious beginning, Peter Rossi provides a thoughtful and careful review of the history of voting research. Just as rewarding are Brodbeck's reflections on irrationality and neuroticism, Key and Munger's skillful disquisition on the relativity of social determinism (undertaken to prevent electoral study from becoming "a non-political endeavor"), Parsons' far-ranging exposition which presents the empirical studies as comforting support for more general theories of social systems, and Pool's dispassionate and controlled excursion into the significance of TV. There are also expert and detailed discussions of thorny methodological issues and

imaginative ventures in adapting techniques borrowed from other fields for the investigation of votes and voters. Indeed, there are very few inconsequential pieces in the entire collection.

Yet, the net impression is a disturbingly disappointing one. No consensus emerges to support the editors' contention that "the discovery of dynamic laws governing the voting habits of a democratic people" is close at hand or that very soon the study of voting behavior may "become the common property of social science." On the contrary, it is apparent that voting behavior remains many things to many disciplines. Able minds reviewing the same data find little agreement as to the prime factors influencing voting choices, the conclusions to be drawn from such a study, or the relevance to central theories of their professions. Conscious and unconscious motivations within the individual voter, pressures generated from primary and secondary group relations, social and economic characteristics of a more general population, institutional and community environment, the impact of various forms of communication all vie with one another as the more significant variables for scrutiny. Inferences to be made for more general propositions about political behavior are similarly divided: some authors find that the voting studies overturn once again the political theorists of the Age of Reason; others discover new hope for rational democracy—especially if the techniques of their own disciplines receive more emphasis. For some, voting evidence contains abundant implications for the general study of human behavior; for others, it is barren of utility beyond the act of voting itself.

Now there is nothing wrong in discovering again that the field of voting behavior is not so well staked out as is often supposed or that the several social sciences remain disparate enterprises. But to the extent that these conditions exist, the function of a collection of essays is to clarify the nature of the differences and to specify the common ground which is apparent. This task, despite the editors' proclamation of "enlarging the frame of reference," the book notably fails to do. For one thing, no control is exercised over the extreme variation in purpose and level of analysis which the chapters exhibit. For another, no order is discernible in the way the articles are arranged and presented. Even

the modest step of arranging the contributions according to the nature of their concern—preconditions, causes, techniques, and implications—is scrupulously avoided.

Thus, highly technical methodological commentaries are interspersed among broader treatments of abstract models; professional articles weave haphazardly in and out between practical injunctions as to how field research may be conducted. Abruptly at the midway point, the protestations of the inevitable single humanist that he is a "lion in a Daniel's den" are introduced (and misreported, parenthically, in the editors' highlight paragraphs as "a Daniel in the lion's den"). As a consequence, the reader is forced to do all the work of synthesis and coordination himself. The book brings neither the field nor its problems into focus; in its presentation, disciplines do not cross boundaries, they simply co-exist.

The most important contribution of *American Voting Behavior*, taken as a whole, then, is a negative one, a sense of a genuine missed opportunity. By its deficiencies, it demonstrates again that really fruitful interdisciplinary collaboration is not the product of neutral toleration in the live and let live sense, or of a policy of "equal time," or of a Noah's Ark principle of "two of everybody." It depends instead on the painful process of each party working in such intellectual proximity that the assumptions, objectives, and methods of the others become known, and common understanding is hammered out.

This is not an easy assignment; in the age of specialization it may not be a feasible one. But it is worth attempting for so important a subject as voting. Until it is tried, humility is the appropriate editorial posture. Flirtations, even when undertaken in sizeable numbers and between book covers, never constitute an affair.

ROBERT C. WOOD

Massachusetts Institute of Technology

The American Communist Party: A Critical History (1919-1957). By IRVING HOWE and LEWIS COSER with the assistance of JULIUS JACOBSON. Boston: Beacon Press, 1958. x, 593 pp. \$6.75.

A literary critic and a sociologist have written the first major analytic and evaluative history of the Communist Party in this country. The scope of the book is deliberately limited in various ways. The underground activities are left out. Public sources have been used and the personal testimony of individuals has not been relied upon.

The periods into which the story is divided are

eminently sensible. There is the period before the formation of the Workers Party as a "legal periscope" and the "plunge" into American politics from 1922 until the early thirties. The process of Stalinization within the American party is said to have been in full swing as early as 1923 and to have come near its climax by 1925. The "ultra-leftism" of the early thirties is succeeded by the "popular front" which made the Party a significant influence in American public life. Then came the blow of the Hitler-Stalin pact and the post-war epoch of "repression and collapse." The tone of the book is indicated by this paragraph near the end:

At least half a million Americans passed through the ranks of the Communist movement and countless thousands of others came under its influence. For nearly four decades the Communist Party exerted a profoundly destructive and corrupting influence upon American radicalism . . . one is struck most of all by the enormous waste of potentially valuable human beings, men who had dreamed of a better world and had been ready to give their lives in order to realize it (p. 499).

In the concluding chapter the authors have undertaken to provide a model for analytic purposes of the transformation through which the movement and the party passed in America and elsewhere after the early period. They express the current interest in discovering the relatively distinctive features of totalitarian politics in our day, an interest admirably exhibited by C. J. Friedrich and Z. K. Brzezinski in their *Totalitarian Dictatorship and Autocracy*. Howe and Coser present a number of propositions—"Toward a Theory of Stalinization"—which are closer to Jules Monnerot than, for instance, to Sidney Hook. Despite obvious reluctance to employ categories that are borrowed from psychology the authors phrase what they seem to regard as their most original contribution in terms like "relative deprivation." They are, of course, contextualists and, for fear of using terminology improper for sociologists and historians, overcome whatever disposition they have to deal clumsily with subjective events.

It is impossible to examine the Communist case history in America without regretting the absence of research that employs the full equipment of the social and behavioral sciences in dealing with the total process. Lacking such material, Howe and Coser have supplied a sparkling collection of revealing incidents and a viewpoint that is clear and open to discount. If this book does not encourage more pointed studies of political radicalism of the right and left, it will be most disappointing.

HAROLD D. LASSWELL

Yale University.

The Seizure of Political Power: In a Century of Revolutions. By FELIX GROSS. New York: Philosophical Library, 1958. xxvii, 398 pp. No price indicated.

This is a thought-provoking book on an important topic. Dr. Gross compares revolutions from an interesting viewpoint—the operational needs of the revolutionists and their opponents—and with the help of illuminating material. To the well-known discussions of the English, French, American, and Russian Revolutions he adds a greater stress on the experiences of the abortive Russian Revolution of 1905, and on the experiences of Poland. The result is a book rich in historical material and with some interesting concepts. The material is not treated with the thoroughness of the professional historian, but rather seen with the eye of a keen and warm-hearted student of politics.

Among the interesting concepts is the "mutation of strikes" from small to large and from economic to political, and finally to a general strike, whether successful or not. A related concept is that of the "fatigue" that tends to set in, Dr. Gross believes, after a short period of strike activity, thus imposing a wavelike character on the whole process (pp. 169–174).

There are shrewd comments on the coalition of classes and the way the Tsar's "October Manifesto" slowed the revolution by appeasing the middle class. The discussions on the importance of a dual power structure, where new revolutionary power centers challenge the previous monopoly of the old ones, are perhaps not new, but they are well taken and worth recording.

The book has some fairly visible weaknesses. It is discursive. Its analytical concepts are neither sharply defined nor consistently pursued. Sometimes the terms seem rather vague, in such phrases as "the psychological impetus of the revolution," and there is a good old acquaintance, "the pendulum was swinging again . . ." (p. 180). Some of the terms sound a bit elaborate: "Revolutionary ecology is the study of the relationship between space and social action in a revolution" (pp. 179–180). Some technicalities have been slighted: there are variant transliterations, fairly many misprints, and some rather strange translations and quotes. Some trivial matters are documented, while some more important and less obvious assertions of fact are not.

Nevertheless, Dr. Gross has presented students of politics and the social sciences with a body of historical reminiscences and ideas that are well presented and clearly relevant, not only to the past history of Eastern Europe, but also to the present and future of the Nationalist

revolutions which are now under way in Asia and Africa.

Perhaps not the least among Dr. Gross' contributions is to remind us that revolutions are a legitimate topic of study for the social scientist. As time goes on, it is even conceivable that the word "revolution" may appear again in the title of a scholarly book. Dr. Gross' publisher was not as bold as all that; the modest subtitle ". . . in a Century of Revolutions" has been omitted from the dust jacket.

KARL W. DEUTSCH

Yale University

The Structure of Freedom. By CHRISTIAN BAY. Stanford: Stanford University Press, 1958. xii, 419 pp. \$7.50.

The mills of the Foundations grind slowly, but they seem finally to subsidize even important ventures. We are in almost desperate need of social and political theory informed by social science and disciplined by philosophy. The crucial problem of freedom, which leads to so many other problems, is examined in this book in terms of the pertinent materials of philosophy and the social sciences. Mr. Bay, in his generous preface, makes the venture—although, of course, it was a lonely one—sound like group research. Clearly, it was aided by Foundations, universities, and many scholars. And that is heartening.

The book, however, can make its reader unhappy. The elaborate apparatus of definition and distinction, bolstered by the paraphernalia of social science, still copes feebly with questions of censorship, the vote, majority rule, and free speech. The need has been felt but not met. Mr. Bay, unlike most scholars, has been educated to deal with a human or social problem, which cannot, because it is human or social, be contained within any one "discipline." He is a product of the Norwegian philosophers gathered around Arne Naess, and of Scandinavian and American social science. His relative failure may still be partly a result of his education, for he has little sense of political realities and genuine possibilities.

The argument of the book turns on the belief that "the maximization of every man's and woman's freedom—psychological, social, and potential—is the only proper first-priority aim for the joint human efforts that we call political." This is so, it sometimes seems, because one can't prove such things and one must start somewhere; Mr. Bay would like to start here. At other times, it seems that freedom is a necessary condition for the realization of all other values, and so has priority. Freedom "means expression of individuality, or self-expression."

There are three kinds of freedom, Mr. Bay contends: psychological, social, and potential. Psychological freedom "means degree of harmony between basic motives and overt behavior," a kind of definition of psychic health. Social freedom "means the relative absence of perceived external restraints on individual behavior." For potential freedom, substitute "unperceived" for "perceived" in the definition of social freedom. Mr. Bay fears that men are manipulated by the press and by advertising, and that they don't perceive the manipulation, thus decreasing their potential freedom.

The chief use of the social sciences is in the discussion of the determinants of each type of freedom. Mr. Bay wants, of course, to maximize each. His reading has been voracious, and relevant.

But the ideas are what count, and many of these are muddled. A single example: Mr. Bay wants free speech for everybody, "for 'neurotics' and 'normals' and for fanatics and skeptics." Even psychotics must be unrestrained in expression (see p. 131). But he is so afraid that people may be manipulated to suit special interests, that he urges (p. 385 n.) "such measures as public subsidies to encourage publications maintaining high standards, whether in journalism or in fiction, and a non-confiscatory but severe taxation of the worst publications." This is to be decided by "groups of outstanding scientists and artists."

The book, then, is a restatement of one kind of traditional liberalism, by means of the new social sciences, but without new ideas or analytical rigor. It is like using the advances of mathematical logic to refine Bentham's felicific calculus. The techniques would be improved, but the Benthamite mistakes and assumptions remain unexamined. So it is here with Mr. Bay's type of classical liberalism.

RALPH ROSS

University of Minnesota

System and Process in International Politics.

By MORTON A. KAPLAN. New York: John Wiley & Sons, 1957. xxiv, 283 pp. \$6.50.

Professor Kaplan describes his work as "an attempt to analyze international politics systematically and theoretically." Many scholars will dismiss the work as another arid, theoretical discussion, conducted at such a high level—and in such private language—as to be completely divorced from reality. To some it will be a belaboring of the obvious. To others it will come as one of the all-too-few recent attempts to develop theory, as sociologists understand the term, in the international field.

Unlike the Snyder, Bruck, and Sapin *Decision-Making as an Approach to the Study of International Politics*, the work is more than a conceptual scheme since it states a body of systematic propositions linking a defined set of variables and giving rise to "distinct empirical research problems." The form of analysis will be familiar to sociologists. It is based on the concept of systems in equilibrium, the processes which may preserve or disintegrate the systems in equilibrium, the values which actors in the system may pursue, and, in a section which uses game theory analysis and whose relevance to the overall structure is not made clear, the strategies which actors may pursue. The author constructs models of six international systems, two of which—the balance of power system and the loose bi-polar system—have actually existed, four of which are hypothetical. These he relates, in terms of hypothetical rules of each system and integrative or disintegrative processes, to other supranational systems (such as blocs), national-actor political systems and their sub-systems (such as parties). The hypotheses, for example, link the pressures of role-conflict on individual decision makers and their tendency to follow the rules of a particular international system.

Reactions to this ambitious, austere, and difficult work will vary as widely as the current approaches to the study of international relations. But every young scholar in the field will find the work on his required reading list for purposes of dissection and discussion.

C. L. ROBERTSON

Smith College

Sociology and the Military Establishment. By MORRIS JANOWITZ. Prepared for the American Sociological Society. New York: Russell Sage Foundation, 1959. 112 pp. \$1.50, paper.

This pamphlet is one of a series sponsored jointly by the American Sociological Society and the Russell Sage Foundation, "each dealing with a single area in which the sociologist is a practitioner or his work is relevant to practice." According to L. S. Cottrell, Jr., who writes the Foreword, the sponsors "believe that it will serve to impress upon sociologists and professional military officials alike the need for more effective utilization of sociological theory and research capability in the analysis of problems of vital importance to our military forces."

Whatever the sponsors would like to believe, this book was not in fact written to "impress" anybody. It is not another selling job. Since World War II, social scientists have tried over and over again to sell their wares to the military establishment. The result has often been over-

selling and a boomerang. The sociologists should instead ask themselves first what they have got to sell. Sociological theory? *What* theory? Anyone who knows the real situation will greet this proposal with the coarse laughter it deserves.

Indeed, this is a far better book than the one its sponsors apparently hoped it would be. It never tells the military that they ought to buy more sociological research. It never tells them how to do their job. It never makes any of the usual pretentious statements about "theory." Instead it describes briefly what sociologists, here and there, have in fact learned about the military establishment, especially how it has been changing since World War II; and where it cannot cite the results of investigation in detail, it refers the reader to the most important books and articles. It makes clear the areas where neither the soldiers nor the sociologists know much; and it explains not only the social problems the military have run into but also the problems with the military that the society should pay heed to. It is written neither for the sociologist nor for the soldier but for the thoughtful citizen, and is a model of how the sociologist behaves when he is really doing his job.

Let me take as an example the author's chapter on "Primary Groups and Military Effectiveness." The increased fire-power and increased complexity of modern weapons systems have killed off the old line of infantrymen, drilled to be dominated as automatons by their officers as they advance upon the enemy. Fire-power forces the men in a unit to disperse, and so isolates the soldier from his fellows. Even more than in the past he must, if he is to be effective, be convinced that he can rely upon them. But the replacement policies characteristic of a mass army tend to break up the primary groups that have gone through training or battle together, and this break-up undermines his conviction. Dispersal has made less feasible the old leadership by domination, at a time when the complexity of weapons makes it more necessary that a unit should act as a team; and both features of modern combat require that the soldier be able to act on his own initiative. All this has tended to render traditional forms of authority, discipline, and training obsolete, not only at the tactical level but increasingly in the style of control exercised at all levels of command. It has naturally provoked traditionalist reactions on the part particularly of officers of middle rank whose status is bound up with the older discipline.

All this the author explains with admirable clarity: what problems in military society the modern technique of war has created, what soci-

ologists have discovered that bears on these problems, and especially what military experience has taught the sociologists. For this book is not just about the contribution sociology may make to military effectiveness but also about the contribution the military have made to sociology. Thus, S. L. A. Marshall's *Men Against Fire* gave a whole new importance to small group research.

I do not mean for one minute to imply that the book is limited to the small, tactical unit. Even when it is talking about the unit, it shows how changes there have affected the whole structure of the military establishment. The main emphasis falls on this larger structure: its organization, its control, its doctrines and indoctrination. The book ends by discussing the limitations of the American military and political traditions in the new situation that heavily involves the military in "peace-time" international politics. The military are not very sophisticated in political warfare, but if they were, how could a democracy control them?

This book does not just ask that more "military sociologists" work for the military. It asks that more plain sociologists study the military establishment as part of their job of understanding our society. It is so sensible that one is delighted to hear that Janowitz will soon publish a longer book on *The Professional Soldier*.

GEORGE C. HOMANS

Harvard University

Small-Town Politics: A Study of Political Life in Glossop. By A. H. BIRCH. New York: Oxford University Press, 1959. vi, 199 pp. \$4.00.

Glossop is a small English industrial town of 18,000 located about thirteen miles from the center of Manchester. In 1953 the Department of Government at the University of Manchester sponsored a study of Glossop on a collaborative basis by a number of persons on the Faculty of Economic and Social Studies. A. H. Birch, Senior Lecturer in Government, has woven the individual studies into a unified volume, which now appears as the book under review.

The investigators used documentary materials, interviewed about eighty people in various positions of influence in public life, sent questionnaires to secretaries of voluntary associations and trade unions, attended meetings of the Borough Council and political parties, and interviewed a random sample of six hundred townsfolk. The total research expenses for all this were, incidentally, only £200.

Any one interested in the political life of modern communities will find this a useful book, and an American will not be disappointed in his

search for significant parallels and differences. Like any other community that has endured for a considerable length of time, Glossop bears the marks of past rivalries and coalitions. During the nineteenth century, as the town grew in population and changed from agricultural village to an industrial town dependent on cotton and paper manufacturing, rivalry developed between the ancient landholding families and the new millowners, a conflict complicated by religious differences. The biggest landowners, the Howards, were Catholics. The leading millowners were either Anglicans and Tories, or Nonconformists and Liberals. As the Howards grew aloof from local affairs in the last years of the century, the two factions of millowners came to dominate political and social life: "they led the political parties, controlled the Borough Council, took turns to be mayor, and competed with each other to represent the constituency in Parliament."

Today all this is changed. The wealthy millowners have been replaced by small entrepreneurs and salaried managers with few roots in the community and less concern. These men belong to the Golf Club and the Rotary but show little interest in the political, religious, or educational life of the town. The main influence over local politics is in the hands of shopkeepers, artisans, civil servants, and school masters. Of the six hundred citizens interviewed, nearly a quarter could not name any one at all when asked, "Who would you say are the most influential people in Glossop?" and the rest gave an average of "rather less than three names each."

Political interest and activity is at a rather low level. Many of the leading citizens, particularly business and professional men, have "little or no contact with the Conservative Club." As for the Liberals, "To show even a mild interest in the affairs of the party seems a sure way to be asked to serve. . . ." For the Labour Party, now the largest on the Council, "as with the Liberal and Conservative parties, the problem is to find suitable people willing to stand rather than to choose between a number of potential candidates." In the sample of six hundred electors, 11 per cent said they were very interested, 33 per cent said they were moderately interested, and 56 per cent said they had no interest in politics.

Although voting is roughly correlated with social class and occupation, the community is too fragmented socially and economically for simple concepts of stratification or class to provide anything more than a crude explanation of political preferences. Historical legacies remain: even among industrial workers, Anglicans vote Conservative over Labour by about 4 to 3, while

Nonconformists and Catholics vote Labour over Conservative by 2 to 1.

ROBERT A. DAHL

Yale University

The Politics of Inequality: South Africa Since 1948. By GWENDOLEN M. CARTER. New York: Frederick A. Praeger, 1958. 535 pp. \$7.50.

In 1948 the Nationalist Party came to office in the Union of South Africa. Since then the Party has resolutely implemented its program of *apartheid* to perpetuate the political and social dominance of the Afrikaner nationalists. Thus, by a kind of paradox, the most vigorous, uncompromising, and politically successful nationalist movement in Africa is European in origin. In other parts of Africa too, nationalist movements are gaining political control, representing the aspirations of the indigenous African peoples, and it is depressingly obvious that a fearful clash is more than likely in the near future.

In this masterly survey of South African politics Professor Carter has analysed the complex background to the rise of Afrikaner nationalism. The disparity in numbers has always affected relations between Europeans and non-Europeans, as they are termed in South Africa. But there are other factors which complicate the situation, notably the struggle for power within the dominant European group between the Afrikaans-speaking and the English-speaking South Africans. In this struggle, embittered by memories of the Boer War, the Afrikaners have the advantage of numbers, and since 1948, the "watershed" year in South African politics according to Professor Carter, they dominate the political situation.

The strength of Afrikaner nationalism lies in the remarkable cohesion of the Afrikaner community, where every social institution reinforces nationalist sentiments. The schools, the Church, the voluntary societies, the local officials, storekeepers, and farmers, all work consciously or unconsciously for the cause. To a certain extent this cohesion is based on fear—of the non-European majority, and, secondarily, of the powerful economic position in the cities of the English-speaking group—and is the reason why every institution in the Afrikaner community has an ultimate political implication.

Apartheid to the more idealistic Afrikaners means a separate territorial entity for the Africans, and at the moment the Government is legislating for it. Unfortunately for the ideal, the facts of the South African economy are against it, for it is utterly dependent on African labor. Moreover, the 11 per cent of South African

territory at present reserved solely for African occupancy (the Native Reserves), which will be the base for the proposed Bantustans, cannot even support at subsistence level the 50 per cent or less of the Union's Africans at present resident there.

The Nationalists have endeavored to mold European society in the Union to an Afrikaner pattern, and to weaken the Africans by reinforcing tribal divisions, despite the continuous contacts of Africans with Western patterns of action and organization. But in spite of the legislative and social barriers erected against African acquisition of industrial, social, and political skills, Africans already constitute more than 50 per cent of the labor force in secondary industry. Thus the color bar is constantly violated in fact, although not in form, and Africans hold down many skilled jobs, although often enough these are down-graded to conceal the facts. Today Africans could cripple South African industry through a general strike, although they are not yet anywhere near the stage of organization that could make this a practical possibility. But all Europeans in the Union are uneasily aware of the potential strength of the Africans. Professor Carter observes that these facts underlie the extended color bar in industry and commerce which protects White labor from the competition of Africans, Coloureds, and Indians, and which is supported with "a vehemence often incomprehensible from outside."

If there is a fault in this admirable book, it is in the rather cursory treatment of non-European political organizations. But as these are not yet effective in the political constitution, and are being ruthlessly whittled down as they arise, this is understandable.

The Politics of Inequality is scholarly, factual, and wise, and is essential reading for an understanding not only of the present South African situation, but of the political development of Africa as a whole. I venture one small criticism: Professor Carter, in describing the African bus boycott of 1957, ascribes the phrase "voting with their feet" to an English editor. Surely this phrase was first used by Lenin?

W. WATSON

University of Manchester, England

In Search of Identity: The Japanese Overseas Scholar in America and Japan. By JOHN W. BENNETT, HERBERT PASSIN, and ROBERT K. MCKNIGHT. Minneapolis: University of Minnesota Press, 1958. x, 369 pp. \$7.50.

For almost a century large numbers of foreign students have enrolled in American universities, and this practice has been stimulated in recent

years by increasing support from governmental and private agencies. It is generally assumed that such visitors derive great benefit from their training, that they are able to utilize their skills to advantage upon their return home, and that they will subsequently become ambassadors of good will. This study is one of a series sponsored by the Social Science Research Council to find out just what does happen to participants in student exchange programs.

The report is based upon two investigations. In one, 45 Japanese students attending two American universities were studied through a battery of standard research techniques. The major part of the study, however, was conducted in Japan, where data were collected on 150 persons who had received at least part of their higher education in the United States. Careers are viewed in historical context, and fascinating contrasts are revealed between the typical experiences of those who went abroad soon after the Meiji Restoration, in the period between the two World Wars, and after 1949. As might be expected, the data show considerable variation in reactions to American campus life, and it appears that whether a person's impressions were favorable or not depends largely upon the extent to which he was rooted in traditional Japanese institutions. Furthermore, a student's fate upon returning home apparently rested not so much upon the quality of his training as upon family connections and upon the state of international relations between the two countries.

Some readers may question the appropriateness of the title, since these students rarely had doubts about being Japanese. The authors contend, however, that the manner in which many Japanese intellectuals identify themselves as individuals depends so much upon their conceptions of nation and race that many personal dilemmas arise from their inability to decide whether Japan should develop along Western or along traditional lines. The difficulties become accentuated for those educated in Western countries, who are able to see advantages and disadvantages in both alternatives. Some of the analytic categories developed in the pursuit of this problem may prove useful to others concerned with the study of self-conceptions.

Although the findings are not implausible, some of them will no doubt come as a shock to people who had taken it for granted that foreign students invariably depart with an admiration for American ways. The volume should prove of value to educators who are planning exchange programs as well as to specialists in inter-ethnic contacts. If intellectuals who set out with the avowed intention of learning see so little of American life, what can be expected of others

who have fewer opportunities to compensate for their ethnocentric impressions through reading?

TAMOTSU SHIBUTANI

University of California, Berkeley

Minorities in the New World: Six Case Studies.

By CHARLES WAGLEY and MARVIN HARRIS.
New York: Columbia University Press, 1958.
xvi, 320 pp. \$6.00.

It is certainly time anthropologists attempted generalizations about minorities, groups heretofore largely studied only by sociologists and psychologists. We may thank Professors Wagley and Harris for suggestive chapters devoted to definitions, to six studies, and to a comparative, theoretical treatise. The disparate histories of Brazilian and Mexican Indians, American and Martiniquan Negroes, American Jews, and French Canadians are compared in detail, assertedly by the historical-comparative method of anthropology, to derive generalizations about factors in minority formation and interaction.

This provocative book reveals the great need for concentrated theorizing about minorities. The authors' five *differentiae specifica* define minorities as (1) subordinate "segments" in state societies, (2) self-conscious "units," (3) "endogamous," (4) disparaged linguistically, physically, or culturally, and (5) recruiting members by a "rule of descent." Unfortunately, these would not adequately differentiate among castes (as the authors admit, p. 10), lower classes, or even regions. These criteria suffer from the use of undefined concepts such as "subordinate," "unit," and "rule of descent." Though all cases studied are treated as "units," nowhere is the "unit" nature of minorities or majorities sociologically established. Furthermore, when "descent" no longer designates kin-group allocations, but any ascription to a group at birth (for example, class, caste, or region), it becomes diagnostically useless.

To define minorities precisely is tantamount to achieving an adequate theoretical description, but definition can only be accomplished after first having rigorously classified structures without regard to their sequential relations. Classification, as the authors' *differentiae* suggest, should involve a taxonomy of intergroup relations, rather than of minorities themselves. By the latter, minorities appear as monads with reified essential characteristics, such as "adaptive capacity" and "cultural preparedness," persisting unrelated to environments. These characteristics, designated "historical-cultural," result from the authors' emphasis on historical treatment prior to taxonomy. However, "adaptive capacity" must either inhere in groups as

such or be a function of the minority-majority interrelationships. In the latter case, it could not be a minority characteristic. In the former, the "capacity," being inherent, would entail temporal and spatial consistency. However, Negro history especially shows drastically varying minority adaptations. "Cultural preparedness" involves similar objections.

A concept, "arena of competition," called an "historical-cultural" factor, designates vied-for socioeconomic values, advantages sought by majorities from the presence of minorities, perpetuation of subordination, and barriers to upward mobility. These "historical-cultural" factors are much more clearly structural with regard to minority-majority relations than are "ethnocentrism" and "endogamy" which are called "structural components" (p. 258). Systematic use of the former for taxonomy would have yielded greater understanding of minorities, as in distinguishing between minority and pluralistic statuses, which the authors identify because they assume that pluralism is less desirable than total absorption—due to the "danger of conflict and . . . subordination" (p. 294). However, just where cultural differences persist in the absence of these "arena of competition" factors, pluralism occurs.

Phases and processes of minority-majority development might usefully have been differentiated by a taxonomy of conjunctive relations associated with societal forms such as folk, peasant, or urban. A classification according to origins might have distinguished among unintegrated, acculturating "tribal" minorities, elaborately integrated but unassimilated "voluntary" minorities, and culturally assimilated and socially integrated "forced" minorities (such as American Negroes). Acculturation and assimilation (and fusion) are conceptually confused with integration and even pluralism, though consistent usage would have suggested assimilation to, fusion with, or pluralism within the matrix society as alternative outcomes whose differential occurrence might reveal cultural regularities. Such empirically based generalizations would illuminate minority formation processes and the authors' contentions that minorities originated only in expanding state societies.

The authors' major generalizations are not really comparative. Historically, the five matrix societies, deriving from a common cultural tradition, possessed kindred socioeconomic, political, and ideological systems. When examining minorities as dependent variables of such societies, similarities are inevitably found. Without comparisons of a single minority, say Jews, in independent systems (like China, India, and America), one cannot know whether similarities are

universal or local minority characteristics. Do minorities universally display upward mobility aspirations as the authors suggest? This suggestion stems more from a value assumption permeating the book ("The trouble is that such cases of upward mobility are relatively infrequent," p. 117) than from any necessary concomitant of minority status.

Personality factors are said not to cause minority-majority relations. Rather, such relations cause personality characteristics. Thus the authors laudably resolve to explain sociocultural phenomena socioculturally, thereby eschewing today's overly-prevalent psychological explanations which leave the psychologically "causal" factors unaccounted for and, consequently, explain nothing. The authors have challenged their colleagues to face the issue of developing a truly sociological theory of minorities.

ANTHONY LEEDS

The City College of New York

Power and Property in Inca Peru. By SALLY FALK MOORE. New York: Columbia University Press, 1958. viii, 190 pp. \$5.00.

United States social scientists are rarely trained in the law; Robert Redfield was an exception that comes readily to mind, and David Riesman is another, but neither has been greatly concerned with blending these interests with his behavioral studies. Mrs. Moore, on the contrary, has attempted in this prize-winning anthropology dissertation to use her legal training in a review of the documentary evidence on Inca law and government.

Given the amount of fancy and idealization pervading the Inca bibliography almost since the European invasion, Moore has defined her task as applying to Inca life the approach which brings Hohfeldian "legal realism" and modern anthropology together: the insistence that in addition to concern for the ideal statement of norms, the student search for the behavioral tests which these norms undergo in daily living. While necessary, this is a difficult job since the civilization under study is known to us only from 16th century accounts by alien soldiers, clergy, and administrators.

Since the behavioral data on most aspects of Inca law and its violation are so fragmentary, Moore centers her investigation on land and tax matters, which European observers could not ignore. As the new way was being installed it stumbled continuously into functioning bits of pre-European land tenures and traditional labor exchanges, which the new bureaucracy recorded and conceptualized. A careful study of the record shows that Inca land law allowed

for more complexity than the traditional tripartite division into peasant, crown, and church holdings. While sharing the standard view that Inca state revenues had their antecedents in pre-Incaic labor services, Moore's own suggestion is that the share of agricultural produce remaining in the provinces, at local discretion, was and continued larger than the contribution made to the Inca center, at Cuzco. This matches her estimates that the ethnic, local rulers kept greater authority and administrative responsibility after the Inca conquest than had hitherto been suspected. She is likely to be right on both counts.

All this is part of a continuing re-examination by anthropologists of the social structures of American "high civilizations" like the Maya, Chimu, Inca, or Aztec. Moore demonstrates that the traditional view of the Inca state as a centralized bureaucracy with paternalistic planning and welfare aims is erroneous. She firmly separates the two distinct sets of law ways and privileges, land tenure patterns, and administrative authority rooted in two co-existing orders: the enduring, pre-Incaic multiplicity of agricultural and ethnic communities vs. the superordinate Inca state. This is not surprising since at the time of its destruction by the Europeans, the Inca state had been expanding for less than a century and its ideological as well as revenue-creating and loyalty-maintaining mechanisms had succeeded only partially in welding together a nation from a congeries of vastly different ecological adaptations, linguistic units, and tribal confederacies.

JOHN V. MURRA

Vassar College

Introduction to Cultural Anthropology. By MISCHA TITIEV. New York: Henry Holt & Co., 1959. xiv, 464 pp. \$6.00.

The author himself provides a brief but perceptive review of his book in the preface. He notes that the book is primarily intended as an introductory college text, not as an all-inclusive encyclopedia; that it does, however, include selected references to other work, both recent and classical; that many points are made only briefly in order to give instructors using the book freedom of elaboration; and that the greater emphasis is on the work of American anthropologists.

In terms of the author's goals the book is a success. Instructors in introductory anthropology looking for an easily comprehensible, balanced, textbook of moderate length would do well to consider adoption.

As a representative introductory text the

book is also worthy of attention as an index of the drift of current anthropological thought. With this in mind a few comments on Titiev's approach are in order.

Titiev holds that most human cultural behavior can be grouped under three major categories, analytically distinguishable but dynamically interconnected. These consist of relationships of man to man, of man to environment, and of man to supernatural or social unit. These appear to correspond closely, in usage at least, to what Kroeber has termed "social culture" (social structure), "reality culture" (technology, economy, etc.), and "value culture" (religion, art, etc.). All three categories are regarded by Titiev as parts of culture, since all involve symbolization, a requisite for culture and the chief mechanism of its transmission.

Space considerations prevent more than a mere listing of other trend-diagnostic features. Among these may be included: a broad definition of culture reminiscent of Tylor's and including artifacts as well as behavior; a recognition of the importance of biological determinants of human behavior; considerable attention to religion (two out of twenty chapters, as compared, for example, to John Gillin's book of a decade ago, which did not even list the word in the index); a matter-of-fact acceptance of the idea of cultural evolution and relative lack of interest in cultural relativism; an assumption of the relevance and at least partial validity of both learning theory and psychoanalysis, and a relative skepticism about reconstructed culture history. Of these features, all except possibly the last can, I think, be fairly regarded as diagnostic of recent developments in American anthropology, to which the book is intended as an introduction.

J. L. FISCHER

Tulane University

Sociology and Social Life. By KIMBALL YOUNG and RAYMOND W. MACK. New York: American Book Co., 1959. xii, 472 pp. \$5.75.

Of the writing of introductory text books there is no end, and perhaps it is well, for the perfect introduction to sociology has not yet appeared; probably it never will. Moreover, different teachers with different students in different settings need different text books. This new volume by Young and Mack appears likely to be most useful for overburdened teachers with large numbers of relatively unsophisticated freshmen. On the whole, the book is clearly and interestingly written and appropriately illustrated.

The Preface sets forth rather well the nature and content of this text. It purports to offer "a way of looking at social life," using a set of concepts ordered theoretically and illustrated "with examples drawn from sound social research." The "four basic concepts" are structure, function, pattern, and process.

Part I, curiously labelled "Social Relations," deals not only with groups, but also with cultural variability, social change, and "structures, functions, and processes which are found in all human societies." Part II, entitled "Social Organization," includes, in addition to stratification and communities, two chapters on population. Part III, "Social Institutions," covers the usual array, and offers a discussion of "the functions, both manifest and latent," which are consequences of these structures.

The last item, a bit of contemporary sociological orthodoxy, bothers this reviewer. Again and again (e.g., pp. 14, 75, 158) we read in effect: In the beginning was structure, after which function emerged. For obvious reasons the case is not well documented, and the reviewer would like to propose an alternative proposition: to meet basic needs human beings developed ways of meeting them (functions) which became stabilized in groups (folkways, institutions, that is, structures). At least in the case of some of more recent structures it can be shown that structure *followed* function.

Another disturbing matter is the apparent confusion of structure and pattern. Thus on page 104 we are told of "structure, or the interrelationship of parts; [and] pattern, a recurring relationship." From the discussion on pages 14 and 15 the reviewer suspects that "structure" is intended to represent an impersonal view of "parts" of a social system, while "pattern" is meant to represent people in action in ways that recur regularly in a given society. But this is not quite clear.

There are several less important criticisms that may be mentioned. The discussion of sociology as a science in Chapter 1 is pretty abstract for beginners. Some material seems almost irrelevant (for example, geologic time and the evolution of man as set forth on page 38 ff.). In some passages quoted from other authors the type is very small (pp. 45 and 127). Some topics are handled in a very sketchy manner (for instance, development of the sense of self, p. 120). In some places there is need of documentation (for example, re performance of school children, p. 281). Plainville is erroneously located in Arkansas (p. 187). In the three population pyramids on page 254 it is puzzling to find changes not only in relative distribution by sex and age,

but also in the *total percentage*. However, these are relatively minor matters.

By and large this is a good piece of work. It should serve a useful purpose for many instructors and students.

STUART A. QUEEN

University of Wichita

Words and Things. By ROGER BROWN. Glencoe, Ill.: Free Press, 1958. xvi, 398 pp. \$6.75.

This is a pleasantly-written, yet basically serious, discussion of a number of crucial or perennial problems about the nature of language and its role in human life. After an introduction to set the stage, and a chapter and a half on the analysis of speech and of writing, the author proceeds to discuss a variety of problems. The latter half of Chapter II takes up the practical issue of teaching children to read. Chapter III, "Reference and Meaning," deals with psychological aspects of the problem of semantics. Chapter IV, "Phonetic Symbolism and Metaphor," asks: Does Phonetic Symbolism exist? Is it everywhere the same or is it a cultural variable? What is the relation of metaphor to sensory description and synesthesia? Chapter V, "The Comparative Psychology of Linguistic Reference," considers human versus animal communication and human communicative behavior under socially pathological conditions. Chapter VI, "The Original Word Game," treats the child's learning of language. Chapter VII, "Linguistic Relativity and Determinism," presents the Whorfian theses regarding the impact of language habits on behavior and thought. Chapter VIII, "Progressions and Pathologies," considers whether there is some single diachronic trend, say from concrete to abstract, in the phylogeny of human communication, in historical change in a language, and in linguistic ontogeny, and whether aphasics and the mentally ill manifest regression on such a scale. Chapter IX deals with "Persuasion, Expression, and Propaganda." Chapter X, "Linguistic Reference in Psychology," analyzes semantic confusions and problems in the technical discussion of psychologists.

Some of these issues, as Brown himself admits (p. 16), are old "chestnuts," viewed by many as no longer worthy of serious investigation. I heartily approve Brown's insistence that there is now enough new empirical evidence to warrant bringing them up for fresh treatment. I find valuable his summaries of recent psychological experiments, and certain other kinds of evidence, of which I should probably otherwise have remained in ignorance. An occasional

passage is extremely stimulating: for example, Brown's discussion (pp. 307-8) of the difference between the referential and expressive aspects of communication is the first treatment of this that has ever made sense to me. For all this, I am grateful.

On the whole, however, the book is disappointing. Brown frequently omits some of the evidence or some of the hypotheses that would strike me as crucial. Time and again, I was led to expect that he was about to hit the nail squarely on the head, only to be frustrated as he suddenly abandoned the task at hand and turned in some new direction. One example must suffice. In Chapter III, Brown brings up the old problem of how one can have a generalized mental image of, say, a triangle, free of all non-criterial attributes such as size, color, or exact proportions. The answer, of course, is that no such general mental image is either possible or necessary. Given the word "triangle," occupying a certain structural position in geometrical terminology and in the whole language of which that terminology is a part, no intervening mental parameter need be sought. The semantic relation between a symbol (be it word or "mental image") and the category of things it represents can be iconic only to the extent that the criterial attributes of the category are picturable in a way involving no non-criterial attributes. To the extent to which this is impossible, the semantic relation must be arbitrary (that is, non-iconic). In language, semiosis is largely arbitrary; therein lies its power. This is the lesson to be learned from the old triangle chestnut, but Brown misses it.

The etiology of the inadequacies of the book, in my opinion, resides in Brown's failure to learn enough about language itself before turning to the various language-related problems that primarily interest him. Chapter I is symptomatic of this. Approximately 26 pages are devoted to phonology, and only six to other compartments of language design. But grammatical systems are as tightly patterned as are phonological systems and bear much more directly on most of the issues with which Brown wants to deal. The ratio should be reversed. Antiquated solecisms about language are scattered throughout the book: language and writing are unforgivably confused (Chapter II); there is uncritical acceptance of an outmoded nineteenth-century typology of languages (p. 259); there is an equally uncritical acceptance of nouns, verbs, and adjectives as among the parts of speech of English (pp. 53, 243); a simple statement of Bloomfield's is flatly misunderstood (p. 270), yielding several pages of straw-man-burning; it is implied that "X" and "Y" are not linguistic

forms (p. 343); the arbitrary figure "300,000 years or so" is offered as the possible age of language (p. 271); there is great carelessness in the citation of words from other languages (at least Chinese, pp. 125, 242).

Brown is to be commended for wanting to write this book; it is only to be regretted that he did not spend a good deal more time in reading and research before letting it crystallize into print. Perhaps, in the not too distant future, we can look forward to a thoroughly revised edition in which the discrepancies between aim and achievement will be considerably diminished.

CHARLES F. HOCKETT

Cornell University

The Integration of Human Knowledge: A Study of the Formal Foundations and the Social Implications of Unified Science. By OLIVER L. REISER. Boston: Porter Sargent, 1958. 478 pp. \$8.00.

For some decades Oliver Reiser has had mankind as a whole in the center of his vision and the furtherance of a "planetary democracy" as his longtime goal. His books, *The Promise of Scientific Humanism* and *A New Earth and a New Humanity*, elaborated this vision and this goal.

Reiser believes that "the failure of our world is the failure of philosophy." The analytical and positivistic temper of much contemporary philosophy is regarded by him as an evasion of the major function of philosophy: "to seek wisdom and provide guidance" by "putting into a unified world-view the findings of the natural sciences" (p. 5). So he has taken as his sustained task the synthesis of a "world-philosophy"—the title of one of his books—in the hope of providing wisdom and guidance.

The Integration of Human Knowledge brings together on a grand scale Reiser's major interests and convictions. It draws into its net such diverse topics as semantics, symbolic logic, mathematics, epistemology, cybernetics, gestalt psychology, parapsychology, the mind-body problem, induction, quantum mechanics, emergent evolution, and the future of mankind. It attempts to integrate such material into a cosmological synthesis as the ground for Reiser's version of scientific humanism, which he thinks may be "the forerunner of a world religion" (p. 460).

While Reiser's language is in the crisp jargon of the day, and references to contemporary science (especially physics) are numerous, the general cosmology which results is historically a familiar one. Reiser notes that his pantheism

is very close to Giordano Bruno's outlook, and he sees similarities in it to some features of Hinduism and to the *logos* doctrine of Hellenistic and Christian thought. His "cyclic-creative cosmology" is explicitly presented as involving a "Pythagorean-Platonic metaphysics of archetypal forms" (p. 373).

Summarily stated, the cosmos is regarded as an infinite and eternal continuum of unmanifest energy which under the guidance of the Cosmic Imagination (or Cosmic Lens) takes on manifest forms at successive levels of emergent evolution, the products of which ultimately return to the continuum of unmanifest energy. The level predicted to next emerge is that of a world sensorium which will be the instrument of planetary man, and to which individual minds will stand in somewhat the same relation as individual cells of the brain stand to the individual brain. Thus a cosmic pantheism is offered as a support for a "pantheistic humanism."

A short review is no place to assess Reiser's argument that his cosmology is supported by contemporary physical science. It is, however, of interest to the readers of this *Review* to note that Reiser makes almost no use of contemporary social science in his argument: "social physics" occurs in the Index but not "anthropology" or "sociology." "Economics, politics, and psychology are missing out," we are told, "because they deal with partial systems rather than wholes" (p. 446). The point is not that Reiser should have done otherwise than he did (for many approaches should be explored, and his is welcome), but rather to underline the fact that other forms of scientific humanism are possible (and to some extent exist) in which support for global ideals is sought in the sciences which deal with man.

CHARLES MORRIS

University of Florida

The Rational and Social Foundations of Music.

By MAX WEBER. Translated and edited by DON MARTINDALE, JOHANNES RIEDEL, and GERTRUDE NEUWIRTH. Carbondale, Ill.: Southern Illinois University Press, 1958. lii, 148 pp. \$5.75.

Wovon Lebt die Musik: Die Prinzipien der Musiksoziologie. By ALPHONS SILBERMANN. Regensburg K.G.: Gustav Bosse Verlag, 1958. 235 pp. No price indicated, paper.

Although music is the great art of the West, and the most modern art, it has been given little attention by British and American sociologists. Weber's book offers one clue to this

strange neglect. As the editors point out, Weber did not develop any theory of the relationship between art and society. His "affective" theoretical category, so brilliantly explored in religious expression, is not developed in terms of art. His personal interest in art was very great. His ability to think concretely in terms of musical form is obvious. His discussion of the social function of the Chinese literati in *The Religion of China* indicates his concern with the power of art in society. Yet even a cursory reading of Weber on art indicates that he did not think well about art.

The basic question about art—its relation to social order—is not answered by Weber because he never asked it. That is, he has no functional theory of art. This is because he cannot relate art to power in society. Social order in Weber is really *political* or administrative order. If, as Weber argued, art has become independent of magic and religion, and yet is not a science, what is it?

What exactly does Weber mean by the rationalization of music? The editors argue that Weber's use of the term "rational . . . has nothing to do with the ends of action, only with the choice of means in terms of their appropriateness to ends." As an example of rationality in affective states the "deliberately calculated rationality" of the female in courtship is offered. The ends of courtship are clear enough, but what are the ends of music? And specifically, what are the rational ends? Weber argues that as musical means become more like mathematics they become more rational, whereas music used as magical incantation produces ends which are stereotyped magical formulae. But this must mean that ends, not means alone, are rational, for if music is used as magic, liturgy, or therapy, and the musical means developed are congruent to these ends, what is irrational about the choice of means?

Failure to deal with such obvious contradiction, and the neglect of all art by Weber, arises from his neglect of art as communication. In this of course, he is no different from Simmel and Freud. Only Burckhardt, and in our generation, Kenneth Burke, keep us close to the *social function* of art. They argue that since society exists in and through communication, and that communication is carried on in symbols given final form by artists, we must study art if we are to study society. They argue too that the "disenchantment of the world" in rationalization through calculation, must be matched by study of what new kinds of "enchantment" are replacing the old.

We need a sociology of music as much to help develop sociological theory, as to create

a new "subject" in sociology. Our social psychology is really the study of man's relation to a contemporary environment. But we know that man acts in time for he is motivated by the past and the future of an act. Sociologists have no theory of what they call "affective experience" simply because they do not accept the *forms* of affective experience as social facts. Unfortunately, Weber's most important excursion into the arts is of little help. But at least he clarifies *why* we are failing in sociology to deal with art as communication of social order.

Silbermann's book is significant because he asks significant questions about the *function* of music in society, as well as dealing with music as a social institution. It is possible of course to study musicians as a "profession" but unless we define the specific function of the profession, we end up knowing more and more about our abstraction, "profession," and less and less about what the profession really does. As we talk about professional stratification while neglecting its effects on the function of the profession, we cannot be sure we say anything about the profession we study that could not be said about criminals, soldiers, or baseball players.

This is a well organized and clearly stated treatment of how the relations between music and society can be reduced to a series of propositions which can be related to the data of musical expression. Because it is a pioneering work, Silbermann's book must clear much ground and stake out a firm site for a sociology of music. As with all German sociological work, it is much stronger on the structural than the functional side. But since the socio-psychological aspects of music are so little developed (in comparison to those of religious expression) we should be thankful for what we have. Silbermann's work should be translated. It would make an excellent text (no slur intended!) and thus make the inclusion of musical sociology possible in schools where our deplorable ignorance of language cripples so much of our teaching.

HUGH DALZIEL DUNCAN

Flossmoor, Illinois

The Religion of India: The Sociology of Hinduism and Buddhism. By MAX WEBER. Translated and Edited by HANS H. GERTH and DON MARTINDALE. Glencoe, Ill.: Free Press, 1958. vii, 392 pp. \$6.50.

With the appearance of this volume, Weber's entire *Gesammelte Aufsätze zur Religionssoziologie* is now available in English. *The Protestant*

Ethic and the Spirit of Capitalism, which includes a translation of the General Introduction to the *Gesammelte Aufsätze*, has recently been reissued in paperback; three of the shorter essays from Volume I appeared in Gerth and Mills' collection *From Max Weber*; and *The Religion of China and Ancient Judaism* have appeared in volumes uniform with the one under review. When this array of monographs is placed beside the collection of comparative studies originally published under the general title of *Wirtschaft und Gesellschaft*, most of which have also by now appeared in English (*The Sociology of Religion* is expected soon), it becomes evident that Weber remains to the present day the pre-eminent worker in the field of comparative and historical sociology.

Reading the immense corpus of Weber's work is like wandering through a giant quarry. One is confronted with great blocks hewn in various stages of roughness or perfection, all evidently planned as parts of a single building. It is in the firmness of that grand design that Weber's greatness is to be judged, not in the flaws and imperfections of detail which have so fascinated his critics. The study translated as *The Religion of India* plays a crucial role in that overall structure.

The long years when *The Protestant Ethic* was the only work of Weber available to English-speaking readers have left a curiously distorted notion of Weber's main thesis. This is the idea that Protestantism and Protestantism alone is the explanation of "modern capitalism." This notion is distorted partly because it fails to see how profoundly Weber viewed Protestantism as inextricably involved in and growing out of the main line of Western cultural development from ancient Israel and Greece. In that Western development it is not only religion in the narrow sense which Weber singles out as contributing to the unique outcome of modern society, but at least three additional elements: Greek rationalism as exemplified in science, philosophy, theology, and literature; the tradition of Roman law; and the civic tradition beginning in the ancient world and continuing down through the medieval cities (on these developments see Weber's Introduction to *The Protestant Ethic, Law in Economy and Society*, and *The City*). A further cause of distortion is the failure to appreciate the strategic significance which Weber discerned in the variable of religion generally. *The Religion of India* is an excellent corrective to these distortions as it illustrates a line of cultural development in marked contrast to that of the West—and one to which religion provides a very important key indeed.

Weber's comparative sociology is drawn on

the broadest possible canvas. On the one side there exists primitive traditionalistic society, a socio-religious unity organized in terms of highly ritualized ascriptive relationships. On the other there is the peculiarly dynamic modern Western society, with its drive toward endless rationalization in all spheres of life, which takes recognizable shape following the Protestant Reformation. But in between are a number of societies which have broken, to some extent, with primitive traditionalism, each taking a somewhat different course of development. One of these is the society in which the Protestant Reformation occurred, not by any means accidentally, according to Weber. India, like China, is in this middle group of societies.

If Weber chose religion as the strategic mode of entry for his comparative studies in general and for this study of Indian society in particular, it is because he found that in religion cultural values, personality motivation, and social structure come together. While not making these distinctions explicit he nevertheless in practice never confuses them and thus renders an analysis of extreme subtlety and complexity. Weber finds the notion of *dharma*, religiously prescribed obligation, to be the core of Hinduism, especially in its deep inner connection with the idea of *karma*, the endless chain of causation working itself out in successive rebirths. The orthodox view is that whatever position one finds oneself in, in this life, is due to the force of *karma* in previous existences, and one's obligation is to fulfill the *dharma* of one's position so that one will improve one's chances in the next incarnation. The intellectuals, revolting against this notion, always sought escape from the wheel of rebirth through some sort of individualistic salvation. Weber shows (p. 146, ff.) how these conceptions hindered cultural rationalization beyond a certain point. On the one hand they contributed to the development of special technologies appropriate to the *dharma* of each profession "from construction technique to logic as the technology of proof and disproof to the technology of eroticism" (p. 147), but at the same time they hindered the development of levels of generalization above the technological because of the fragmentation involved in the notion of occupational *dharma*. On the other hand the intellectuals were so completely preoccupied with the problem of salvation that all philosophy was made subservient to this end.

With respect to social structure, it is the *dharma* concept as integrated with the idea of caste, which is the key to the situation. In spite of the remarkable achievements, including economic achievements, of certain castes, there is always a limit imposed by the traditionalistic

definition of the caste *dharma* itself. Further, the division of society into innumerable watertight compartments, while engendering a very stable integration of sorts, allows a minimum of flexibility and especially limits the generalization of political power, making the society a relatively easy prey to foreign conquest. The major religious movements which reject this mode of social organization either fall back into it in the form of a new caste, or, as in the case of Buddhism, remain an individualistic and socially negative group existing symbiotically in relation to traditional society and unable to generate any really different mode of social organization.

In terms of personality, the *dharma* idea results in the fragmentation of response directed to the external demands of ritual obligation on the one hand, or a passive withdrawal into mysticism and asceticism on the other. There is no basis for an inner unification of personality for action in response to the command of a transcendental God. The Indian alternatives tend to be action without unification or unification without action.

These remarks serve only to suggest the subtlety and power of the main line of Weber's analysis. In addition there are innumerable riches in his treatment of particular movements and phases, such as his handling of the organic ethic of the Bhagavadgita (p. 180, ff.), of the significance of King Asoka (p. 235, ff.), or his penetrating comments on Japan (p. 270, ff.).

Critically there is of course much to be said but, without claiming to be an expert in this field, I would suggest that it is more in matters of detail than in the main line of analysis that Weber is vulnerable. There are the usual annoyances of disregard for chronology, statements made as though general which only hold for certain periods, minor errors of fact or interpretation. It is in view of this kind of thing, which admittedly does exist in much of Weber's work, and which has often been allowed to obscure the major contribution of his work, that the translators and editors of this volume must be criticized. A careful editor can catch and correct these minor errors through editor's footnotes. Gerth and Martindale not only fail to do this, they do not even catch the proof errors of the German text, which are numerous, and, worse yet, they introduce very many new errors into the English text. And on top of a careless editorial job, the translation itself is unreliable. There are serious mistakes in the translation which either distort Weber's meaning or turn it into its opposite, some of these occurring in really important passages. For example, Weber's delicate discussion of the difference between

"miracle" and "magical spell" (pp. 335-336) is hopelessly garbled. Minor awkwardnesses of translation are very numerous. For example *seelsorger* is generally translated as "soul helper" or "soul benefactor." Weber's German expression for "B.C." is translated "before our time calculations" (p. 301), and so forth. All transliterations of names or words from Sanskrit, Chinese, Japanese, or Arabic are to be mistrusted. Bibliographical references are frequently wrong. The student should not yet throw away his German edition. In this book we are only in the Brill stage of the translation of Weber. Our James Strachey is yet to come.

In spite of all handicaps the influence of Weber in American sociology is clearly rising. More than one book has been written on the inspiration of a footnote or a paragraph from that great corpus. Marianne Weber in writing the preface for the last volume of the *Gesammelte Aufsätze zur Religionssoziologie*, consisting of the fragmentary study of *Ancient Judaism*, but lacking the projected studies of Talmudic Judaism, Islam, and Early Christianity, says in the name of her dead husband, "*Was ich nicht mache, machen andere.*" That is our challenge and our inspiration.

ROBERT N. BELLAH

Harvard University

Magic and Religion: Their Psychological Nature, Origin and Function. By GEORGE B. VETTER. New York: Philosophical Library, 1958. 555 pp. \$6.00.

At a time when books, periodicals, and organizations devoted to the scientific study of religion are appearing in substantial numbers, one picks up a book with the title listed here with a good deal of anticipation. Unhappily, the reader is doomed to disappointment. The book is poorly written, argumentative rather than analytic, and weakened by a number of misconceptions.

There will be wide agreement with many of the observations of the author: that objective study of religion is very scarce, that public media of communication treat traditional religion tenderly, that application of basic psychological knowledge can help one understand religious phenomena. It is this last point which is the basic thesis of the book. In chapters entitled "Some Popular Explanations of Behavior and Religion," "Sex and Religion," "Mind and Religion," "Magic and Religion," "Learning and Religion," "Belief and Faith," "The Religious Experience—Mysticism," "Priesthood," "Social Movements and Religion," "Contemporary Magico-Religious Manifestations," and "Ethics

and Religion," Vetter introduces various psychological evidences to show how religious experience can be explained naturalistically. The author does not, however, review the psychological research carefully. He uses the illustrative method to "prove" a point, makes numerous assertions without any supporting evidence, and develops a rather limited view of contemporary psychological theory without any indication that other psychological points of view are widely held.

These difficulties seem to stem from the fact that the author is at least as interested in writing a tract as an objective study. His opponents, the "animists," the "theologians," or others who are naive—that is, take a different view from the author—are gently reproached: "Come, come gentlemen . . . I have news for you." The news is often an assertion with little or no supporting evidence: "This writer has no evidence whatever on the efficacy of fees in the therapeutic process. But he is highly confident that a sizeable fee will stack the cards in favor of the treatment getting credit . . ." (p. 283). "A large percentage of Westerners believe that the priests of India or Arabia have fantastic powers at their command enabling them to defy ordinary mundane laws" (p. 353). "So the clergy can claim but at best a small fraction of the high I.Q.'s where once they claimed, practically without competition, the whole" (p. 57). There are many such propositions in the book, many of them deserving attention; but they are given simply as alleged facts, without supporting evidence.

Anthropologists and sociologists will find many surprising statements in the book. For example: "But it has long been apparent that most aspects of anything that could possibly be called religion must at bottom be psychological phenomena" (p. 3). This reductionism is the more surprising because the author makes a great deal of use of the concept of culture. ". . . the perennially made error of social theorists has been that of underestimating the varieties of social patterns that can develop and even survive for long periods . . ." (p. 291). This is an interesting reversal of the more common objection that social theorists contend that "the mores can make anything right."

In the last analysis, this seems to be not a book about religion, but a religious book, an expression of Vetter's faith in science: "Scientific method, as a source of ideas worthy of belief, is hardly two hundred years old. And look at the magnificent structure that it has raised! A religious faith less than ten times that age would have to be set down as a relative upstart, a Johnny-come-lately" (p. 266). And

the author has faith that his religion will prevail, for the efforts of "the priesthood" ". . . must in the long run be rear guard actions" (pp. 384 and 445). Perhaps most readers of the *Review* have a touch of Comte in them, but this study is unlikely to strengthen their convictions. One might welcome a serious religious book written from the premises of "materialism" (a term Vetter uses frequently), or a careful application of contemporary psychology to religious phenomena. The mixture one finds in *Magic and Religion*, however, produces a badly blurred picture.

Lack of space permits me only to mention that there are many interesting pieces of information in the book, some awkward figures of speech that a little editorial work might have improved upon ("Under communist needling, the tide has turned"), and vigorous if somewhat unsupported discussions of such widely variant topics as population pressure, eugenics, semantics, and political movements.

J. MILTON YINGER

Oberlin College

Religious Behaviour. By MICHAEL ARGYLE. London: Routledge & Kegan Paul Ltd., 1958. xii, 196 pp. 25s.

This is at once both a highly useful and peculiarly disappointing book. It brings together the empirical evidence on religious practices and beliefs in England and America which has accumulated in the behavioral sciences over the last 40 to 50 years. The evidence is organized around the principal independent variables—for example, environment, age, sex, personality, social class—which have been postulated as related to such dependent variables as church membership, frequency of attendance, attitudes toward religion, and religious beliefs. There is a final chapter reviewing existing "theories of religious behavior and belief."

The utility of the book lies in the fact that it does bring together between the covers of a single book a great variety of material on the social psychology of religion, much of it from relatively obscure sources. The author has been most conscientious in covering the literature and as a consequence has produced a reference book which almost everyone interested in the study of religion will want to have at hand.

The book is nevertheless disappointing, partly because of the way that the author has treated the material but more, perhaps, because most of the material itself proves to be unrewarding. One experiences a sense of defeat that after so many years of effort the social sciences should have learned so little of significance about the

religion of contemporary man. In large measure, the fault is a conceptual one—the failure to develop dependent variables which would adequately order the religious experiences, beliefs, and practices of individuals. Such indicators as church membership, frequency of attendance, the recitation of prayers, and belief in God hardly seem a sufficient basis on which to make the necessary distinctions. Too, the lack of imagination shown in the choice of independent variables is discouraging.

It might have been hoped that, in his useful effort to synthesize the material, the author would also have provided the kind of constructive critique which would point the way for future research in this area. Regrettably he has not done this. Judging the book on what he does try to do, namely to produce a synthesis, its greatest weakness perhaps is an uncritical acceptance of results of studies varying widely in the quality of their design and methodology. Only where it is relevant to a point which the author wishes to make (e.g., p. 28) is the methodology of any study questioned. Questions of reliability and validity, or of the generality of the findings, are given passing reference in the introductory chapters but are not followed through in the presentation of the material itself.

The organization of the book—with each chapter being given over to the discussion of one or several independent variables—serves to encourage description but to discourage analysis. Thus, the mutual effect of several independent variables on a dependent one is rarely considered. However, this may be as much a consequence of inadequacies in the materials as a result of the way the book is organized.

These criticisms notwithstanding, the book is a welcome addition to the literature. It informs us as to where we stand and, if read thoughtfully and critically, may help to stimulate a fresh attack on this important area of human behavior.

CHARLES Y. GLOCK

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Religion in America: Original Essays on Religion in a Free Society. Edited by JOHN COGLEY. New York: Meridian Books, Inc., 1958. 288 pp. \$1.45, paper.

This paper-bound volume consists of eleven essays based on papers delivered at a seminar on Religion in a Free Society sponsored by The Fund for the Republic in May, 1958. Contributors include John Courtney Murray, Reinhold Niebuhr, Leo Pfeffer, Wilbur G. Katz, Will Herberg, James Hastings Nichols, Walter J.

Ong, Stringfellow Barr, Gustave Weigel, Abraham Joshua Heschel, and Paul Tillich.

Although given a relatively general title, the book is concerned with a specific subject: the major problems created by the existence of varying religious viewpoints and religio-governmental interaction in the United States. Dealing with such a subject, the book should be of interest to all but the most specialized sociologists. But, unfortunately—from your reviewer's point of view—the book will appeal to only a few in the ranks of sociology. There are at least two reasons for this (aside from the ponderous writing style employed by most of the essayists).

First, the essays are quite frankly the value judgments of intelligent and well-read social and religious observers who are concerned with "setting the record straight" on their own point of view and on their view of others. This means that the essays are not "covered o'er with a scab o' symbols;" one contributor relates that he deliberately resisted the temptation to marshal statistical data to "prove" one of his points. Thus, the essays violate the modern sociological canon that writers should use empirical data even if their efforts amount to little more than what has been termed the statistical elaboration of the obvious.

Second, the essays implicitly challenge the so-called objectivity with which most contemporary social scientists—including, in the past, your reviewer—treat religion. This "objectivity" is all too often a thinly disguised antipathy to organized religion and to non-materialist philosophies. Your reviewer is not here pleading for "belief," but simply expressing the opinion that general sociology and the specialty known as the sociology of religion are desperately in need of practitioners who are genuinely objective about religion and not unconsciously or consciously advocates of the modern cult which is best described as "dogmatic secularism."

The inadequacies of the dogmatic secularist approach to religion—and, I repeat, most modern sociologists, as sociologists, are dogmatic secularists—is revealed in a number of ways by the essays in the volume being reviewed. The contributions of Murray, Niebuhr, Herberg, and Ong, for example, point up the unique and basically important contributions made to American culture by its diverse religious organizations; they also show that the really important leaders of American Catholicism and of other large groups are more often than not democrats to the core and not at all the narrowly-educated victims of an outmoded philosophy. Such points can hardly be appreciated by the dogmatic secularist who says and feels that he is "objective" because he is "not religious." Thus, the

volume will probably not be read or understood by those sociologists who could profit most by it.

THOMAS FORD HOULT

Wayne State University

Popular Religion: Inspirational Books in America. By LOUIS SCHNEIDER and SANFORD M. DORNBUSCH. Chicago: University of Chicago Press, 1958. ix, 174 pp. \$4.50.

This book is a demonstration of what happens when apparatus overpowers idea. The idea is sound enough: to describe one side of middle-class religion in America through a close examination of forty-six best-selling inspirational books published between 1875 and 1955. The apparatus, consisting of elaborate content analyses and an overdeveloped sociological commentary, only succeeds in obscuring this interesting material.

The results of the survey, largely presented in Chapter II, are for the most part what one might expect. The best-sellers share a pragmatism, optimism, activism, and individualism which have often been said to characterize American intellectual life generally. In these books, religion is seen as a means to the goods of this world: success, happiness, prestige, world betterment, riches (stressed in earlier books), emotional security (stressed in later books), and so on. Man is good, God is available, subjective experience is praised over ritual and dogma (though less so in recent years), and positive thinking is the technique for the only salvation that concerns us—that which occurs in this world.

Having traced out these and similar themes in some detail, and illustrated them with some excruciating quotations from their authors, Schneider and Dornbusch push on, in Chapters III, IV, and V, to their interpretive commentary. This portion of the book seeks to provide a sociological context for the themes uncovered in the reading of the best-sellers, but it pursues this worthy aim amidst so much tiresome and irrelevant pretentiousness that one wishes the book had remained a journal article.

The authors contend, on the whole unsuccessfully, with assorted problems in the sociology and philosophy of religion. The former are treated with a curious combination of reverence and naivete which suggests an unfamiliarity with the issues. (The problem of magic, for instance, is treated most obliquely.) The latter (for example, the "noble lie" problem) are, to be sure, peripheral to a sociological study; despite the authors' frequent admonitions on the neutrality of science, however, they permit

themselves to be drawn into covert arguments with their material. Impelled, for some reason, to take up the cudgels against Norman Vincent Peale and his ilk, they indulge themselves in scoring little points with a patronizing smugness that ill becomes either the scientist or the social critic. The inspirational literature may be nonsense but it is at least pathetic nonsense: the world of widows' rooms which it inhabits requires more understanding than it receives here.

Despite the pages devoted to sociological commentary, the most important failure of this study is its inability to place its problems in the contexts of the American and the Protestant traditions. Some references are made to the presence in the inspirational literature of New Thought (prominent) and the Social Gospel (negligible), but the wider intellectual and social contexts receive little treatment. William James is frequently referred to but since Schneider and Dornbusch look upon him as an "exemplar" of inspirationalism, one's confidence remains unaroused and the void remains unfilled. Similarly, their conception of Protestant sectarianism as a "thinning" of churchly Catholicism—a bias which reflects Sociologism rather than Romanism—does little to inspire respect.

Finally, a comment is in order on the content analyses. The code consisted of 193 categories (not counting 44 division groupings which could also serve as categories). Abandoning a paragraph-by-paragraph scoring after coding thirty-one books, the authors turned to rating whole books in terms of the code. The authors declare themselves pleased with the reliability of various coders and procedures. I cannot share this optimism, but in any event our disagreement is irrelevant since the enormously cumbersome data-collecting apparatus remains largely unused—the authors gracefully claim this necessity as a virtue—and only general tendencies are noted in the text.

Popular Religion raises disturbing visions of some inherent affinity between sociological elaborateness and shameless pedantry.

MICHAEL S. OLMSTED

Smith College

Modern Revivalism: Charles Grandison Finney to Billy Graham. By WILLIAM G. McLOUGHLIN, JR. New York: Ronald Press Co., 1959. vii, 551 pp. \$6.50.

Revivalism has been subject to diverse interpretations. Believers frequently see revivalism as the revitalization of an unhealthy religion. "Non-believers" believe revivalism to be symptomatic of a sick social order. McLoughlin, however, sees revivalism to be part of the periodic

process of re-examining and redefining the social and intellectual values of a society. The religious aspects of these re-examinations have been called "great awakenings." It is in this context revivalism is studied. Implicitly, McLoughlin sees these re-examinations as being functionally important in maintaining an equilibrium between stability and change in a society.

The first of the great awakenings, from 1725 to 1750, was exemplified by George Whitfield. It was a reaction against the liturgical and ecclesiastical legalism of the transplanted European Churches and resulted in sectarianism and pietism. The second of these awakenings, from 1795 to 1835, is the point of departure of the book. This phase is dominated by Charles Finney who succeeded in completing the destruction of Calvinistic pessimism and replacing it with a more Arminianized optimistic theology. This process had its parallels in the rise of Jeffersonian and Jacksonian democracy. Finney also provided a rationale for harnessing the forces of frontier revivalism into the church system of America's growing cities. Believing that men did not have to remain passive about their souls, salvation could be achieved in the same way that men could achieve political liberty. The Protestant clergy turned from being shepherds of their flocks to being ranchers concerned with rounding up lost souls.

The third awakening, 1875-1915, was typified by Dwight L. Moody and Billy Sunday and was oriented toward the country bred and the intellectually unsophisticated caught in the shift from an agrarian to an industrial society. This was a time when Protestantism was liberalizing and adjusting to these changes but the revivalism of this period was a reaction against this readjustment.

The fourth awakening, currently under way, is typified by Graham. Both his neo-fundamentalism and the emergence of neo-orthodoxy have provided different reactions against the optimism of "liberal" Protestantism shaken by crises.

McLoughlin describes these last three awakenings with accuracy and meaning. The reader sees the threads of continuity from Finney to Graham in their techniques, their protracted meetings, and their pattern of attack. There is no doubt that these awakenings can only be placed in the context of a more general social and intellectual re-examination. McLoughlin, for the most part, is content with a description of the religious changes and rarely attempts to relate them specifically to the more general social and cultural changes. Neither does he explain why the first two awakenings were almost revolutionary while the last two were almost reactionary. This is particularly puzzling since re-

vivals have seemingly been encouraged by a similar segment of the social structure, the already-churched lower middle class.

McLoughlin sees revivalism as an integrative force in American society since sectarian bickering has been minimized for the greater task of saving souls. In doing this, however, revivalism has secularized Protestantism—by reducing theology to efficiency, by reducing salvation to pietism, by reducing human problems to personal problems, by seeing religion as a panacea to all problems, and by reducing Christianity to Americanism.

The reader is constantly aware of the ironies implicit in revivalism. Revivalism emphasizes the importance of what one believes and then ignores theology and fosters anti-intellectualism. Objections to revivalistic techniques are countered by advice "not to argue with success." Revivalists justify their new techniques as keeping the church up to date while they explain the need for their services as a result of the churches' departure from old tried techniques. Revivalists insist that spiritual regeneration is an individual matter and then try to regenerate collectively. While emphasizing personal reform, revivalism's advice is to conform—conform to a narrow moralistic formula. This was recently exemplified by Billy Graham's preference for dignified hand-holding in the parks of Moscow as compared with the more expressive love-making in London's Hyde Park. By this very observation, the irony that individual morality is no index to institutional morality is observed but not seen, at least not by Graham.

This book is an excellent social history, as was the author's earlier book, *Billy Sunday Was His Real Name*. For the sociologist, it is perhaps unfortunate that his theory of revivalism is not more fully developed and the similarities of these four periods are not made more explicit. But perhaps these are tasks for the sociologist. In finishing the book, one is left with the sad reflection that, with the increasing organizational efficiency of American society, the virtues of religion can seldom be as well organized as its vices.

RUSSELL R. DYNES

The Ohio State University

Great Basin Kingdom. An Economic History of the Latter-Day Saints, 1830-1900. By LEONARD J. ARRINGTON. Cambridge: Harvard University Press, 1958. xviii, 543 pp. \$9.00.

Interest in the Mormons seems to be a hardy perennial. The volume under review is a significant and definitive contribution to that interest. The volume is divided into four parts.

Part I, "Design of the Kingdom (1830-1846)," deals with the initial economic programs until the forced departure of the Saints from Nauvoo, Illinois. During this period "seven basic principles"—as the author calls them—emerged: (1) the organized departure of the faithful members from the wicked world, (2) a planned community design with the farm land located beyond the periphery of the village, (3) the "Stewardship" system under which "the property rights and holdings of the Mormon villagers were to be allocated and regulated by the church . . . (p. 25), (4) the redemption of the earth, that is, the development of natural resources which was "regarded as a religious as well as a secular function" (p. 25), (5) "frugality and economic independence" (p. 26), (6) the widespread use of a program to unify and consolidate the Saints through various collectivistic schemes, "for every realm of economic activity" (p. 27), and (7) equality in distribution, that is, "land and other properties were to be allotted" in the words of the founder-prophet, Joseph Smith, "equal according to their families, according to their circumstances, and their wants and needs" (p. 28).

Thus it was that very early in the history of this movement, spiritual evangelism was closely linked to "economic activity under church direction" (p. 33). There was a wedding of theocracy with a controlled economy not unknown in the history of some of the early American colonies.

Part II, "Building of the Kingdom (1847-1868)," discusses the solutions which were sought for a variety of problems: cultivation of arid land under irrigation, the church control of immigration through the "Perpetual Emigrating Fund," and the settlement and further extension of economic and community planning—all aimed at isolation and self-sufficiency. Yet, as the author points out, the California gold rush and particularly the development of mining—largely under non-Mormon (Gentile) aegis—limited the attainment of these aims both in scope and time.

Such limitations are further elaborated in Part III, "The Kingdom Threatened (1869-1884)." The completion of the transcontinental railroad and the Mormons' own railroad building destroyed the former community isolation. But the threat of non-Mormon competition along economic lines was met by the establishment of cooperative merchandising and by the establishment of "The United Order of Enoch." The latter called for planned cooperative communities, in some of which private enterprise was tolerated alongside community-wide cooperative programs of farming, trading, and

manufacturing, while in others, "Settlers contributed all their property to the community United Order, had no private property, shared more or less equally in the common product, and lived and ate as a well-regulated family" (p. 333).

The United Order was the most extreme effort of the Mormons to attain economic and social self-sufficiency but in the end the pressures from the outside proved too great. But the efforts at cooperation did promote thrift, permit the accumulation of capital with which to purchase machinery and equipment, and aided in the development of natural resources.

The book closes with Part IV, "Kingdom in Retreat (1885-1900)." The long campaign of non-Mormons both in Utah and outside to liquidate polygamy was finally accomplished. Polygamy was actually but a convenient "evil" to secure non-Mormon aid from outside in the fight against the economic and social autarchy of the church. The disfranchisement of the church by federal court order and the forced surrender of its financial assets to a court-appointed receiver proved a serious blow to their economic and social control over the Great Basin.

The former program for community isolation and economic self-sufficiency was abandoned and while the Mormon church continues to own banks, stores, and industries, it operates within the framework of the contemporary profit system. The vestige of former cooperative programs in the so-called "Welfare Plan" means very little in the total economic life of the Saints.

For the sociologist this book is of interest as a cultural case-study of the effects of planned economic self-sufficiency on social organization and community life.

For the social psychologist it has data—at least at the implicit level—on problems of individual adjustment to cooperative programs in a world which stresses individualistic enterprise and interpersonal competition. The basic problem was that of a society with an essentially authoritarian, mercantilist economy in conflict with a larger American society based on political democracy and a profit-seeking economic system.

The volume is an important addition to Mormoniana. While O'Dea's *The Mormons* (1957) is an excellent though brief history of the Saints, it is derived largely from secondary and easily available sources. In contrast, Arrington's well-written book represents years of tedious plowing and searching among extensive and sometimes obscure original sources. We are all in his debt.

KIMBALL YOUNG

Northwestern University

Water Witching U. S. A. By EVON Z. VOGT and RAY HYMAN. Chicago: University of Chicago Press, 1959. xi, 248 pp. \$4.95.

Vogt and Hyman have subjected to the objective scrutiny of scientific analysis a behavior pattern that is widespread, deeply imbedded in folklore, and ardently championed. In so doing they have managed to be interesting, informative, and even significant—and they have kept their sense of humor.

The basic questions around which the book is organized are: "What is water witching?" "Does it work?" "What makes the rod move?" "Who is the water witch?" "Under what conditions is it practiced?" and "Why does water witching continue to be practiced in the United States?"

For those who have never met or heard about water witches, water witching is a means of locating ground water with a forked twig cut from a peach or willow tree (or, for that matter, with a crowbar, coathanger, horsewhip, shovel, pitchfork, or pendulum). The witch paces slowly over the land holding his "instrument" in his hands in one of several positions. When a supply of ground water is detected the forked twig, or whatever, dips violently toward the ground—apparently without volition on the part of the witch. A first-rate witch can not only locate water, but can tell you how deep you will have to drill to get to it! Most witches take little or no fee, and there is no evidence that many are purposeful charlatans. The practice originated in Europe about 400 years ago, and diffused rapidly to other parts of the world, including colonial America.

To answer their basic questions Vogt and Hyman turn to a wide variety of sources: historical documents on the origin and diffusion of the practice, interviews with and observation of water witches in various states, correspondence with water witches in many parts of the world, newspaper clippings and magazine articles, data on field experiments, detailed case materials, and materials on related phenomena such as the tapping table, talking horse, ouija boards, and magic pendulums. They also sent 500 questionnaires to a stratified sample of county agricultural extension agents.

It will not come as a shock to most readers that there is no substantial evidence from anything resembling controlled experimentation to support the notion that water witches locate ground water with anything more than chance frequency. What is interesting is that the practice is so widespread and is supported by so much case material. Apparently even careful observers exhibit an extremely high degree of selective perception. And the convictions of both observers and diviners are so strong that nega-

tive evidence is quickly rationalized out of existence.

Vogt and Hyman relate the frequency of the practice to urbanization, level of education, and difficulty of finding ground water in the ten major ground-water regions of the United States. They are consequently able to place the practice in the same category as Eskimo shamans, the American Indian Ghost Dance, and the rain-making activities of Hopi kachinas: it is a magical ritual that functions to allay anxiety by making the threatening unknown more predictable.

The author of this review has used the book in introductory anthropology as a case study of a culture pattern to illustrate the functions of magical ritual (along with materials on Arunta increase ceremonies), as a demonstration of the nature of self-sustaining hypotheses (as all cultural belief systems are), and to demonstrate the fallacy of dependence upon anecdotal case materials as scientific data. Students in this course find *Water Witching U. S. A.* particularly telling because it is a pattern from our own cultural fund. More sophisticated readers may find the book relevant to their interests for the same, and other, reasons.

GEORGE D. SPINDLER

Stanford University

California Cult: The Story of "Mankind United." By H. T. DOHRMAN. Boston: Beacon Press, 1958. xi, 163 pp. \$3.95.

One of the classical procedures in sociological research is the *post facto* analysis of a social movement. H. T. Dohrman's case study of "an economic-religious-utopian" cult that grew up, flourished, and declined in California between 1934 and 1951 lies squarely in this tradition, not only in subject-matter, but also in method and point of view.

Dohrman assembled and assimilated what he could find in the way of published material: the cult's "bible" or textbook, newsletters and bulletins, occasional directives from its head to followers, records of a few meetings, and the transcripts of testimony at hearings and trials when the cult became involved in legal difficulties (the investigation of its alleged "un-American activities," and bankruptcy proceedings). In addition, the author interviewed the cult leader and 74 members or ex-members at length. He makes no claim about the representativeness of his sample of members and it is unquestionably not systematically drawn.

The conceptual equipment of the study is also classical. In a three page appendix titled "Notes on Theory," the author refers in passing to

verstehen sociology, which probably best describes his approach. He also mentions some of Weber's ideas and *anomie*. But Dohrman's approach to the study does not incorporate anything that can be accurately called theory. This lack is the greatest weakness of the study as it is of so many other efforts in the same tradition.

The absence of theory not only precludes any kind of hypothesis-testing, but results in a diffuseness of purpose, a lack of pointedness in the questions asked (and the answers given) about the movement, and a general impression that only the surface of the problem has been explored. At the end, one may feel that he has been exposed to a "slice of life" but that he has no more fundamental a grasp of the dynamics of events and people involved than he did at the start.

The author gives some attention to questions of personality and motivation in relation to participation in the movement, but the conclusions he reaches are disappointing: the "typical" cult member is or has been a rebel against religious orthodoxy; he has "done a lot of thinking about" religion; he is intellectually eclectic and participates in many cults simultaneously or sequentially; he is middle or lower-middle class, and relatively poorly educated; he is inquisitive and intellectually frustrated and often lonely and economically insecure. Some of these conclusions are open to question simply because of the unrepresentative sample on which they are based; others are of doubtful generality because they are based on members of only one cult, therefore not justifying the author's use of such expressions as "the cult follower" and "the cultic mentality." But, even disregarding their validity and reliability, these conclusions are not penetrating; they suggest rather than solve questions of why and how participation in cults comes about.

Dohrman poses a question that the general public often asks about cult members—"They're abnormal, aren't they?"—and answers it in the negative. By this, the author seems to mean that the standard psychiatric categories do not fit the observed behavior of cult members. From Dohrman's observations one can agree, but surely cult members are deviant in their attitudes and beliefs. One could argue that while they show ordinary intelligence, cult members often display extraordinary credulity. This phenomenon is worth exploring and it is not enough to dismiss it by saying simply that they are not psychotic.

Aside from these general reservations, Dohrman has done a workmanlike job. The cult he chose has intrinsic interest, a manageable, if far from small, size, and somewhat more adequate

documentation than many others. The author's writing is perceptive and his case histories of two participants are insightful and suggestive.

HENRY W. RIECKEN

University of Minnesota

The Psychology of Sexual Emotion. By VERNON W. GRANT. New York: Longmans, Green & Co., 1957. viii, 270 pp. \$4.75.

This book seems to be a labor of love and its topic is love. Unfortunately this is not a sufficient basis for making a contribution to the topic. Grant is head of the Department of Psychology at Hawthornden Hospital in Macedonia, Ohio, but has utilized for this book little or none of his own clinical experience. Instead, the work seems to have grown from a lifetime of reading on the matter of attraction between human beings, from which the author has culled an immense number of quotations and comments relative to many aspects of sexual and emotional attraction, largely between male and female.

He has organized these materials very well. His earlier chapters are devoted to a classification of sexual attraction or love, and he attempts, quite properly, to separate various elements in it, such as imagination, esthetics, sexual desire, frustration, and so forth. He then takes a rather wide perspective and presents many views of love, from many well-known authors, such as Havelock Ellis, Freud, Spencer, and Binet. He deals with amorous behavior among the primitives, as well as amorous sexuality in ancient Greece.

The last sections of the book are devoted specifically to the problem of choice, in which he focuses upon both psychological and sociological factors.

What is missing throughout the book is any line of theory, or even any re-evaluation of well-known ideas. Instead, the book is a compilation of divergent and often interesting opinions on all these topics, with no reconciliation of opposing points of view. It is useful to have these comments brought together, and some will be able to use the author's references productively. He has dug up many items which one might easily overlook if one were attempting a more systematic or creative presentation.

WILLIAM J. GOODE

Columbia University

Sex Without Guilt. By ALBERT ELLIS. New York: Lyle Stuart, 1958. 200 pp. \$4.95.

Ellis is a psychotherapist with a background in clinical psychology, who has written abundantly on various aspects of sexual behavior in

modern society. This book brings together a series of articles which he wrote a few years ago for the *Independent*. They are essentially short sermons. Each sermon takes up various types of sexual behavior and urges us to stop feeling guilty (under certain conditions) about indulging in them. These practices include petting, pre-marital sexual relations, adultery, sex without love, homosexuality, and a few others.

Although Ellis insists that his is a completely factual approach, and he urges a freedom of choice upon us, in fact his arguments are about as value-laden as the attitudes and values which he so vigorously and eloquently opposes. Consequently, the discussion rarely rises above the level of a parlor discussion among close friends. The complexities of the topic are omitted or ignored. There is, for example, almost no recognition of the essentially social and cultural origins of the attitudes which he opposes, and thus no recognition of the great problems which the execution of his suggestions would create. He tells us in effect not to feel guilty, but this, too, is overly simple, since our guilt is not based on facts, but upon our prior rearing. Moreover, even if our guilt were based on facts, Ellis has no facts—and probably no one else has either—which could demonstrate that either behaving as we do, or behaving as we might, would be more efficient for society or more healthful for the individual.

Possibly for those whose attitudes are unthinkingly opposed to all these practices, the sermons will be usefully shocking.

WILLIAM J. GOODE

Columbia University

They Walk in Shadow: A Study of Sexual Variations with Emphasis on the Ambisexual and Homosexual Components and Our Contemporary Sex Laws. By J. D. MERCER. New York: Comet Press Books, 1959. 573 pp. \$5.95.

The author of this book calls himself a "self-confessed and self-accepting ambisexual." Yet this reviewer learned that the name is a pseudonym, "self-confessed" apparently referring to self-insight rather than to public disclosure. A book about homosexuality, authored by an American homosexual freely revealing his identity, is yet to be written.

The book assembles a vast quantity of material on male sexual variations, much of it being quite familiar to scholars and clinicians. "Mercer" is a layman but he does not hesitate to criticize many of the psychologists and psychiatrists who have specialized in the sexual field. He fully realizes that both the nature and the

incidence of homosexual patterns display the widest possible variations. He has probably devoted many years of observation and study to a subject that has seared his soul. Indeed, a spirit of passionate dedication permeates the entire book. "Basically, we ambisexuals and homosexuals do not want to be 'strange.' We want to be accepted, not with sentimental sympathy, for a 'disability' or a 'disease' which we must disclaim, but with some measure of knowledge and understanding. We want to be free of archaic legal prohibitions which incorrectly mark us in the eyes of the world as criminals."

The intolerance of *mores* and laws toward homosexuals is especially glaring in a society where respect for individual rights and differences has been a stated ideal. Yet one can disagree with some of the arguments brought to bear upon this thesis. For instance, the author refuses to consider the ambisexual abnormal when, from the statistical evidence presented in pages 636 to 659 of the first volume of the Kinsey research, he certainly seems to be so. "Abnormality" means nothing more than deviation from a given norm and can be used by scientists as a descriptive term without connotations of disparagement.

This book would undoubtedly be more effective in accomplishing its avowed purpose of enlightening the general public, judges, law-enforcement officials, and legislators if it had been edited down to one-half its present size. Yet some of the chapters, notably the one entitled "A Legal and Social Blueprint for the Future," are written with telling pungency and contain much food for thought. A technical glossary, a slang vocabulary, and a bibliography are appended.

CLAUDE C. BOWMAN

Temple University

The Criminal Mind: A Study of Communication Between Criminal Law and Psychiatry. By PHILIP Q. ROCHE. New York: Farrar, Strauss & Cudahy, 1958. xi, 299 pp. \$5.00.

Dr. Roche has written a challenging study, but since his book deals with the criminal mind as such only inferentially, the prominent place which it occupies in the title is somewhat of a handicap in the author's communication with the reader.

It is Dr. Roche's belief that the criminal law has a very different concept of crime and the criminal than does psychiatry and that this difference in conceptualization inevitably leads to the use of different symbols and therefore makes communication between the two disciplines difficult, if not impossible.

The author traces the descent of the present legal concepts of intent, premeditation, and malice from their older derivatives in the concepts of personal demons that inhabited the individual. With such an orientation, crime is seen as something actuated in isolation within the individual. Another concept that he sees as basic to criminal law is the notion of free will, which implies the responsibility of the psychopath. Dr. Roche sees criminal law as arising from the public's unconscious fear of contact with the criminal as the instrumentality of the forbidden wish. The criminal trial then becomes a religious ceremony in which the hidden wish is publicly disavowed. In such a ceremony, the role of the psychiatrist is to act as moral inquisitor, a role which was formerly played by the theologian. Being essentially a religious ceremony, the criminal trial is a rigid ritual in which the moral parables of our child-rearing practices are reiterated with the judge taking over the parental role of powerful, punitive agent.

As a psychiatrist, on the other hand, Dr. Roche is aware of the intimate relationship between neurosis and crime. The mental hospitals he believes to be filled with people whose latent crimes are lived out in symbol and allegory. He states baldly that "there is not a single inmate in the penitentiary who does not suffer with a disturbance of communication with his surrounding reality, which is to say, that the distinction between a mental hospital and the penitentiary is largely a matter of terminology." This leads him to the consideration that if the law were to come to the view that all felons were mental cases, there could then be a reformation in penology in which the criminal would be forced into self-awareness. The law at present allies itself with the criminal in his resistance to facing the idea that crime is a form of mental illness.

With two such opposing concepts of crime, what are the possibilities of improving the communication between the two disciplines? The author takes some hope from the fact that both law and psychiatry have a common goal in that both are devoted to dealing with their fellow men. He suggests that the psychiatrist can best fulfill his scientific function out of public view during the pre- and post-trial periods. He feels that the maximum utility of the scientific data can be exploited in the following administrative contexts: "1) advisory on the question of trialability of the accused, 2) informative and advisory leading to appropriate disposition of the convicted, 3) provision of techniques for changing convicted persons in the direction of self-awareness and reform, and 4) advisory on questions of release of convicted persons."

The book, then, is an attempt to apply scientific principles to the treatment of crime and the criminal. It is all the more remarkable, therefore, that Dr. Roche has limited himself almost exclusively to the psychiatric approach to the problem and that the words psychology and sociology do not even appear in his index. Perhaps one of the reasons that the psychiatrist is assigned to a purely religious role in the conduct of the trial is that too often the psychiatrist acts as a member of a remote priesthood with insufficient experience in communication with his fellow scientists. The problem of crime and the criminal is too broad in its etiology and symptomatology to be the exclusive domain of any one discipline.

HAROLD GREENWALD

New York City

Work and Society. By EDWARD GROSS. New York: Thomas Y. Crowell Co., 1958. vi, 652 pp. \$6.75.

This is a solid and authoritative book which will do much to stimulate the development of occupational sociology as a distinctive specialty, and especially as a subject of instruction.

It is divided into four major sections, the first of which sets out the careful definition of the field and analyzes the interrelation of work and life-patterns in two exotic societies—Dobu and rural Ireland. The next section describes the major types of occupation in the United States—professional, white collar, and blue collar, with emphasis on institutional systems and the patterning of mobility. The chapter on "The Career" is the best summary of the state of knowledge concerning occupational inheritance which has appeared in print. The third section treats work organizations, using the farm, the restaurant, and the factory as examples. A final section, appearing almost as an afterthought, gives brief treatment to labor unions and ethnic relations in industry.

Both the merits and the demerits of this book are those of the field which it summarizes. Perhaps the chief merit is interpretive coherence. The underlying agreement among occupational sociologists on theoretical questions is really quite remarkable. Such issues as have arisen—on managerial bias or the mechanisms of labor force redistribution—stand at the fringes, not at the center. Another merit is the impressive accumulation of descriptive knowledge.

As Gross shows, much more is known about the basic facts of occupational life in the United States than is generally realized. The available empirical data compare favorably with those on urban or family institutions, for example, although the latter have had much more socio-

logical attention. Finally, Gross carries on the happy tradition of borrowing useful data from economics, psychology, and anthropology without a trace of parochial feeling.

The principal demerits of the field itself are also reflected in the book. The gravest is lack of structure. The world of work is vast and ramified. The development of both theoretical and empirical knowledge has been very uneven. By sheer chance, a great deal more is known about restaurants than department stores. There have been excellent studies of vertical mobility in the professions, and almost none on promotion in bureaucracies. This problem haunts Gross, like other writers in the field, at every turn. The farm, the factory, and the restaurant, considered as work-organizations, do not match the professions, the blue-collar, and the white-collar occupations, treated as institutional systems. Moreover, what is known about the farm is superficial and dubious compared to what is known about the factory. But we must take our fields as we find them. An accurate survey can not be criticized for irregularities in the terrain.

THEODORE CAPLOW

University of Minnesota

The American Labor Force: Its Growth and Changing Composition. By GERTRUDE BANCROFT. For the Social Science Research Council in cooperation with the U. S. Department of Commerce, Bureau of the Census. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1958. xiv, 256 pp. \$7.50.

One of the Census monographs, this volume carries out admirably the intent of the Social Science Research Council in sponsoring the series. The author defines her purpose, in part, as bringing up to date the pioneer efforts of John D. Durand in the Council's publication, *The Labor Force in the United States, 1890-1960*, published in 1948. One of the obvious differences between the two volumes results from the growing wealth of current statistical information in this field. The characteristic feature and great virtue of the new volume is the detailed and critical use of this great mass of new data. No one has greater familiarity with these statistical materials than the author.

Miss Bancroft does much more, however, than simply present in convenient form a descriptive summary of labor force developments. She is primarily concerned with isolating the factors underlying the changes which are taking place in labor force behavior in terms of propensity to work. These cannot be explained as the mechanical effects of demographic changes

alone. By the approach made familiar by Durand, Goldfield, Jaffe, and Wolfbein, it can be shown that shifts in age structure, marital status, and farm-nonfarm residence do not go far to explain—except for white males—changes in the propensity for active participation in the labor force.

Much attention is devoted throughout the volume to explaining the marked increase in the propensity of white women to work and, what the author regards as more significant if not puzzling, the marked differences with respect to this tendency between white and nonwhite women, and white and nonwhite men. The need or desire for income is not a satisfactory explanation. According to the author, "the types of persons whose propensity to work increased most over the decade were not those who were in the lower socio-economic groups."

Many clues are given that suggest a hypothesis in terms of job opportunities and a process of selection on the part of both employers and employees. Some weight must be given to the effect of World War II on customs and attitudes; but in this connection it is interesting that the author has abandoned for purposes of projections the use of a cohort technique used by Durand and by Wood and others.

The author finds more significant that "the increase seems to have been greatest in those areas where earnings of men were highest, and presumably the demands were pressing for women to take some of the lower paid jobs that men would not fill." The suggestion is made that nonwhite males have both rejected jobs and been rejected for jobs: their participation rates show the greatest decrease at both ends of the educational scale. The lack of attractive job opportunities for nonwhite women may explain why they stay out of the labor force if family incomes permit. However, as in the case of white women, the more years of education the more likely it is that nonwhite women will be in the labor force.

The volume is largely concerned with analysis of secular tendencies of this kind. For this purpose the author uses all the data at hand, conforming the decennial census data to Durand's adjusted historical series to take advantage of the special data available once in ten years, but using the more reliable current sample results without adjustment for complementary analyses. The tables are clearly labelled "decennial census" or "current survey" levels and the reader has little trouble on this score. The appendixes on "Quality of census labor force data and methods of adjustment" and "Some problems of concepts and measurements" are recommended to

students who want a sophisticated introduction to the statistical data in this field.

This reviewer is disappointed that Miss Bancroft confined her analysis largely to secular developments of this kind and passed over the problem of cyclical changes in the growth and composition of the labor force. In a brief section the author suggests a job opportunity hypothesis to explain cyclical fluctuations but, in a summary sentence, leaves it open to doubt whether in fact there are cyclical fluctuations.

CHARLES D. STEWART

U. S. Department of Labor

The Blackcoated Worker: A Study in Class Consciousness. By DAVID LOCKWOOD. London: George Allen & Unwin Ltd.; Fair Lawn, N. J.: Essential Books, 1958. 224 pp. \$3.80.

This book is a contribution to the study of social stratification and occupational sociology—and in this instance, to the sociology of the British clerk, the typical blackcoated worker. The study is sharply focused on the class consciousness of the clerk and seeks to study the factors affecting his sense of identification with, or alienation from, the working class.

There is a significant historic setting. In the last seventy years the number of clerks has increased more rapidly than that of any other comparable occupational grouping. Today, one out of every ten workers in Britain is a clerk. It is said that the clerk has come to share the same class position as the manual wage earner. He is divorced from the ownership and control of his means of production. He is "propertyless" and obliged to sell his labor power to make a livelihood often at a price below that of manual workers of lesser education. In spite of the expectation that the clerk would align himself with the working-class movement, he has remained aloof or lukewarm to trade unionism. Labor critics have charged that the failure of the clerks to define accurately their own economic position has led them to a false consciousness of their position. They have called the clerks the "white collar proletariat," to emphasize the "pathetic" self-deception of the blackcoated worker who was seen as indulging in middle-class pretensions on a working-class level of living. The degree of correspondence between what clerks believe their class position to be and what their class position actually is constitutes the consistent and recurring theme of the study.

The researcher gathered evidence to depict the facts of class position by examining the *market situation*, the *work situation*, and *status situation*. He found that the income differential between manual and non-manual workers has been

largely closed. However, the clerk has been generally better off than the manual worker with regard to hours of work, conditions of work, security of tenure, pension schemes, and similar benefits. His chances to rise to managerial positions are somewhat better. He finds that the work situation has been changed in varying degrees by the growth of bureaucratic administration. Impersonality and divorce of the worker from the product has occurred but not in any sweeping fashion. The still relatively small size of the units of production helps maintain small working groups where they are in personal and cooperative contact with management. The status situation is marked by ambiguity. The middle-class status claim of the clerk has been slowly undermined during the rise of the modern office. The growth of universal literacy, the recruitment of clerks from lower social strata, the gradual transformation of office work into predominantly women's work, and the increased emphasis laid on productive contribution have all adversely affected the prestige of the blackcoated worker. Yet in certain socially relevant aspects (such as the high proportion enjoying secondary grammar school educations) the clerk continues to be distinguished from the manual worker.

For all of these reasons, only about one out of every four clerks belongs to some kind of trade union. The writer believes that the traditional superiority of non-manual work has not been entirely eradicated and although there has been a loss of some middle-class status, this has not led the clerk to define himself as a member of the working class. It is true that the class consciousness of the clerk now exhibits greater diversity, with the bank clerk at a middle-class limit and the railway clerk at a working-class limit. As a group, clerical workers are now more heterogeneous and the sheer fact of brain work is less and less the hallmark of middle-class status.

This research is entirely descriptive. It explores not the occupational life of the clerk but his class position and class consciousness. It is concerned mainly with the role of the clerk in relation to the trade union movement. One looks in vain for comparisons that might be drawn with other industrial nations. There is not a single reference to C. W. Mills' *White Collar*. Mills deals with the work situation of the clerk, the status situation, and unionization. It seems incredible that the Atlantic Ocean can still isolate our British colleagues from our writings in the United States. Of course, this is not completely fair because Dr. Lockwood cites such Americans as Centers, Bendix, Lipset, and Veblen. He just didn't read Mills or he didn't think his study called for comparative evidence.

Anyhow, the book constitutes another part of the long-term research into social stratification which is being carried out at the London School of Economics. It is now in the realm of public property and the search for general principles makes the book valuable. For anyone interested in the larger problem of the new middle classes, comparative evidence is now accumulating.

DELBERT C. MILLER

Pennsylvania State University

Sozialer Aufstieg und Abstieg: Eine Untersuchung über Berufsprestige und Berufsmobilität.
By KARL MARTIN BOLTE. Stuttgart: Ferdinand Enke Verlag, 1959. ix, 253 pp. DM 28.—, paper.

Among the most promising developments in contemporary sociological research is the rapidly growing number of social mobility studies. Since World War II empirical research in this area has been conducted in a number of countries. The resulting publications are now beginning to add up to a cumulative body of knowledge of international scope. The theoretical significance of this work and its comparative orientation represent welcome departures from the triviality and parochialism characteristic of much empirical research.

The volume under review offers a substantial addition to the developing body of evidence. It reports a comprehensive attempt to measure the extent and degree of occupational mobility in the West German province of Schleswig-Holstein. A special effort was made to assess the effects of a whole series of upheavals—depression, Nazism, war, defeat, and economic recovery—upon the social structure of the area. Since Schleswig-Holstein received a heavy postwar influx of East German refugees and "expellees" whose lives were more drastically affected by the turbulent political events than were those of the native-born, the occupational history of both groups was investigated separately.

As a preliminary step, a study of occupational prestige was undertaken. Some 1600 persons, mainly students in secondary vocational schools in Kiel and Hamburg, were asked to rate 38 occupations. Consensus among the raters was high and the resulting scale differs very little from similar studies of occupational ratings conducted in various countries. Schleswig-Holstein's occupational prestige hierarchy closely resembles that of other German provinces and of other nations.

The occupational prestige scale was used to measure both intra-generational and inter-gen-

erational mobility. The data were derived from personal interviews with all persons resident in the province in 1953 who had been born on certain days during the years 1893-1901 and 1913-1921, resulting in a total of 1750 interviews. It was thus possible to compute mobility rates separately for two different age groups, for natives and refugees, and for four different time periods, encompassing prewar, war, and postwar years.

The major findings are as follows: (1) The dramatic historical events had no lasting impact on the occupational structure of Schleswig-Holstein. The shifts which have occurred were mainly caused by basic trends toward increasing urbanization and industrialization which were interrupted only temporarily by the war. (2) There have been no major changes in the occupational mobility patterns of the population. To be sure, the war and its aftermath caused many individual dislocations, especially for the older refugees, but the postwar occupational movements of all groups strongly resemble the prewar patterns. Again, the major changes stem from the underlying industrialization of the area. As a consequence, the younger age groups show higher mobility rates than the older ones. (3) The increase in mobility rates is only partially accounted for by shifts in the occupational structure. When structural factors are held constant, the evidence indicates that Germany's classes are gradually becoming more "open." (4) Finally, some international comparisons made by the author are worth noting. He confirms the recent findings of Lipset, Bendix, and others that the overall patterns of social mobility appear strikingly similar in various industrial societies. But Bolte shows that this holds true only as long as comparisons are limited to three crude categories of manual, non-manual, and farm occupations. When finer subdivisions are introduced international differences in mobility rates increase. Thus, if a five-fold occupational classification is used, the degree of social mobility still appears significantly higher in the United States today than in Germany.

It is a pleasure to recommend this study which has been executed with consummate care and great skill. The author, a student of the late Professor Mackenroth at Kiel, ranks high among the small group of younger scholars whose efforts to emancipate German sociology from its traditional speculative orientation through systematic empirical research have begun to bear solid fruit.

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BOOK NOTES

Culture and Society: 1780-1950. By RAYMOND WILLIAMS. New York: Columbia University Press, 1958. xx, 363 pp. \$5.00.

The concept of culture as intellectual and imaginative achievement of high quality first gained currency in England at the beginning of the nineteenth century. In origin it was a response to the social transformation effected by industrialization and to the ideological reorientation inspired by the Liberal dogma of the French Revolution. The efforts of sensitive minds to come to terms with these changes subsequently developed the concept into an elaborate complex of ideas. The components of the complex included subsidiary notions, such as the superior reality of art, the perfectibility of society through aesthetic creation, and the very special nature of the artist; they also included cognate concepts, such as culture as a preserve of a "cultivated minority" and culture as a "whole way of life."

Raymond Williams has painstakingly charted the development of this complex through its major permutations and shifts of emphasis. Beginning with Burke and ending with Orwell, he assesses the successive contributions of a long array of thinkers, such as Coleridge, Arnold, Ruskin, and T. S. Eliot. In each case he carefully relates the man's views on culture to the central tenets of his thought. All this he does with skill and judgment. His work should prove of value to specialists in the sociology of knowledge.—J. JEAN HECHT

Eilert Sundt: A Pioneer in Sociology and Social Anthropology. By MARTIN S. ALLWOOD. Oslo: Olaf Norlis Forlag, 1957. 112 pp. No price indicated.

This little book gives a portrait of Eilert Sundt (1817-1875), Norwegian philanthropist and social reformer, and a pioneer in the social sciences in Scandinavia. As a young student of theology serving his internship in a prison in Kristiania (now Oslo), Sundt came in contact with Gypsies and soon developed a more than curious interest in these strange wandering people, an interest which he later extended to other marginal and under-privileged groups, such as the peasants and the slum dwellers of the city.

Throughout it is clear that Sundt was deeply concerned with the plight of these people: "... but in order to find out how one could really do something about it, I felt that I should

have to have a much more thorough knowledge of their conditions . . ." (Sundt as quoted by Allwood, p. 24). Although deeply romantic in his background, Sundt avoided the idealistic glorification of the "primitive" and tried to reach an objective understanding on the basis of carefully observed factual data, making extensive use of statistical methods. In this way he became the first systematic demographer and ethnographer in Scandinavia.

One recognizes the spirit and method of Frederic Le Play, although there is no reference to Le Play's work in the present volume or even an indication that Sundt was aware of his French contemporary. Instead, an attempt is made to present Sundt as a theoretical sociologist, which is hardly justified.—PETER A. MUNCH

The Study of Leadership. Edited by C. G. BROWNE and THOMAS S. COHN. Danville, Ill.: Interstate Printers and Publishers, Inc., 1958. 487 pp. No price indicated.

Because the field of leadership is far from organized, the editor of a pertinent anthology must range widely in his selection of material. Browne and Cohn, in their collection of articles, have chosen wisely from an enormous literature and have been properly eclectic.

Although this is a first anthology in the field of leadership, there has been no attempt to give a history. If such a temptation existed it was probably easily suppressed, since there are as yet no pivotal concepts in the field which might stimulate historical interest regarding their origins. The studies and essays are mainly of recent vintage; of the 51 articles, more than half were written in the last 8 years and only 6 prior to World War II.

Where the articles were lengthy, the editors have abridged them intelligently. References used in the original articles have been retained, thus indirectly offering a fairly large reference source for those interested in reading further. It would appear that the major function of this book would be to serve as a text in a course on leadership or to provide outside readings in a course where leadership is one of the topics considered. Without pretending to organize the field (although the articles are grouped into four major categories), this anthology is a useful survey of the literature and an excellent source for the non-specialist.—HARVEY BURDICK

The Educational Characteristics of the American People. By SLOAN WAYLAND and EDMUND DE S. BRUNNER. New York: Distributed by Teachers College Bureau of Publications for Bureau of Applied Social Research, Columbia University, 1958. 258 pp. \$3.00, paper.

This analysis was planned as one of a series of monographs presenting the results of the 1950 Census, prepared under the auspices of the Social Science Research Council. (Because it did not meet the time schedule set for the series, it is not, like the others, published in regular book form.)

The data are organized in age categories covering the elementary, high school, college, and adult years; they are analyzed by regional, rural-urban, sex, and racial differences, and, for adults, are related to such factors as marital status, fertility, mobility, income, and occupation. The monograph contains a detailed treatment of the data on 5, 13, 15, 17, and 21 year old age groups and a discussion of the adult population in terms of factors associated with the amount of education received. There is a final chapter on the characteristics of American teachers. In some cases, comparisons are made with the 1940 Census data, and with 1900.

The document is a useful reference work primarily for educators, but also for sociologists. The data are clearly summarized, the discussion is brief and straightforward, and the interpretations are well drawn.

One point is of special interest: the tremendous gain in educational attainment of successive generations of Americans—the average American urban young adult today has completed high school, while his parents on the average had completed eighth grade—has not occurred by a series of regular small steps. Instead, the authors conclude, "In a sense, the educational attainment of our people gives a picture of successive plateaus . . . with the ascent from one to the other . . . a relatively slow process much of the time with sudden spurts caused by such social phenomena as depressions, world war, and its aftermath of federally subsidized education for veterans."—BERNICE L. NEUGARTEN

Early Education of the Mentally Retarded: An Experimental Study. By SAMUEL A. KIRK. In cooperation with MERLE B. KARNES, RAY GRAHAM and WILLIAM SLOAN. Urbana: University of Illinois Press, 1958. 216 pp. \$6.00.

This is a report of a five year experiment on the effects of preschool education on the mental and social development of 81 mentally retarded children between the ages of three and six. The results are most encouraging, primarily because

the investigators employ an extensive case analysis of the children and their environment. Mentally retarded and defective children can progress within their limitations through proper school experiences. It should be emphasized that while educational stimulation is not a cure for mental retardation it can improve the functioning level of many children if experienced early.

Of the 43 children who received preschool education in the experiment, 30 revealed an acceleration in rate of growth which raised their classification from one to three levels. And while it was difficult to improve the rate of growth of organic defectives, 50 per cent of these children made some improvement.

The need for a wholesome psycho-social home environment is indicated. To this reviewer, however, the importance of the contribution lies in the fact that society's responsibility for mentally retarded children from impoverished emotional environments is clearly pointed up. This study is bound to stimulate much constructive thinking and acting in the field.—ARTHUR LERNER

The Social Welfare Forum, 1958. Official Proceedings, 85th Annual Forum of the National Conference on Social Welfare, Chicago, Ill., May 1958. New York: Columbia University Press, 1958. xii, 311 pp. \$5.00.

Although "Accent on Prevention" was the theme of the 85th Annual Forum of the National Conference on Social Welfare, only 6 of the 19 articles in this volume have any direct bearing on this somewhat elusive phenomenon. Papers by Roney and Hunt emphasize the obvious importance of early intervention to prevent economic dependency and personal and family disruption. Reid's article on the family is largely inspirational and exhortatory, while McRae (now president of the National Conference) boldly offers the social worker the "new" but necessarily vague role of "social educator-statesman," and urges the profession to recapture its "ardor for reform" and to prevent "disorientation" on the societal level.

Bernard's paper on "Neomarital Programs," dealing explicitly with the problem of preventing marital breakdown, proposes a number of social innovations. Using Welcome Wagon hosts and hostesses, special group discussions, and a network of referral agents for counseling, therapy, and a somewhat controversial "surveillance" technique, Bernard hopes to reach the statistically vulnerable neomarital couple in order to offer the support and guidance formerly supplied "by kinship and spontaneous small local groups."

Those who like to snipe at social work will find a storehouse of ammunition in Burns' article chastening social work for its "cocoon approach" and "professional myopia." Her suggestions for dealing with poverty and dependency should attract the attention of economists and sociologists teaching courses in social security and social pathology. Rural sociologists will be especially interested in Dunham's article on the "Outlook for Community Development;" social psychologists will find Wiltse's "The Hopeless Family" and Frank's "Change Through Group Experience" genuinely creative attempts to extend and elaborate Erikson's ego identity concept into a group context. All social scientists interested in crossing disciplinary lines will be stimulated by Coyle's competent construction of "The Bridge Between Social Work and the Social Sciences," by far the longest paper in this volume.

This volume should be highly useful to social workers and social scientists alike as a source book of current trends and issues.—HOWARD J. PARAD

Medical Care Needs and Services in the Boston Metropolitan Area. By LEONARD S. ROSENFELD, JACOB KATZ, and AVEDIS DONABEDIAN. Boston: United Community Services of Metropolitan Boston, 1957. xii, 147 pp. No price indicated.

Based on interviews with respondents in a sample of 1,066 families as well as on data from the 1950 Census, this careful study is one of a series designed to explore various practical methods of measuring unmet need for medical care in a community.

Indirect support for use of the "symptom survey" method is found in several kinds of evidence, including demonstration of an inverse correlation between socio-economic status and unmet need as indexed by untreated symptoms. Statistical correlations between Census data and interview items also show that for certain purposes it is feasible to substitute socio-economic characteristics of census tracts for those of families. The report presents a wealth of information about the health status, needs, and practices of individuals in the sample. As is common in epidemiological studies, findings are analyzed according to standard demographic categories and attention to social dynamics is largely eschewed. Nevertheless, the authors note that "Studies of understanding of hygiene and health needs, attitudes and motivation . . . would be useful" (p. 88).

Of obvious methodological value for researchers in public health, this report should also interest sociologists who are concerned with social

stratification, community problems, or patterned health behavior.—M. E. W. G.

Demographic Yearbook, 1957. By UNITED NATIONS, DEPARTMENT OF ECONOMIC AND SOCIAL AFFAIRS. Ninth Edition. Special Topic: Mortality Statistics. New York: United Nations, 1957. International Documents Service, Columbia University Press. viii, 656 pp. \$6.50, paper; \$8.00, cloth.

This is the ninth in the highly useful series of demographic yearbooks published by the United Nations. Taken together with the 1951 volume, it offers historical series of mortality data for a twenty-year span, from 1936 to 1956. In addition, several new mortality tables are added. An introductory chapter of 12 pages discusses "Factors in declining mortality" with particular reference to recent experience. Other tables not presented annually show data on migration and on the population of major cities. It is unfortunate that the cumulative guide to the special tables published in preceding issues, included in some of the earlier volumes, has once again been omitted.—G. F. M.

World Population Pressures. By HAROLD L. GEISERT. Washington, D. C.: Population Research Project, The George Washington University, 1958. 46 pp. No price indicated, paper.

World Population Pressures is the introductory publication in a series of short studies dealing with the interrelations of "social and cultural with demographic and economic characteristics in the technologically less advanced societies." It seems to be addressed primarily to policy-makers and the general public, although some of Geisert's appraisals are sufficiently provocative to arouse the interest of professional demographers and sociologists. A descriptive survey of world population growth is followed by a brief review and appraisal of widely postulated causal sequences in population growth. These theories are in turn examined for guidelines to policy. The direct transferability of historical Western experience to contemporary problems of underdeveloped areas is viewed with a healthy skepticism. On the whole a judicious, if elementary, essay.—LEIGHTON VAN NORT

Problems in Intercultural Health Programs. By GEORGE M. FOSTER. New York: Social Science Research Council, April 1958. v, 49 pp. Fifty cents, paper.

This essay is one of several memoranda commissioned by the SSRC's Committee on Preventive Medicine and Social Science Research. The first part briefly outlines cultural and social prob-

lems involved in planned efforts to spread medical information and health practices across cultural boundaries. It stresses the role of theoretical knowledge about directed culture change in helping to solve such problems. The second, longer part considers ways in which existing knowledge may be utilized in program planning and operation, and suggests a variety of topics

for research. Throughout, the essay draws primarily on anthropological data in order to formulate and illustrate pertinent principles. Within the interdisciplinary audience addressed, those who are already familiar with such data will find little new in the essay; those who are not may discover helpful and attractive vistas of information.—M. E. W. G.

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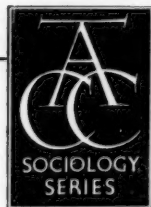
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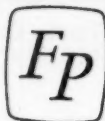
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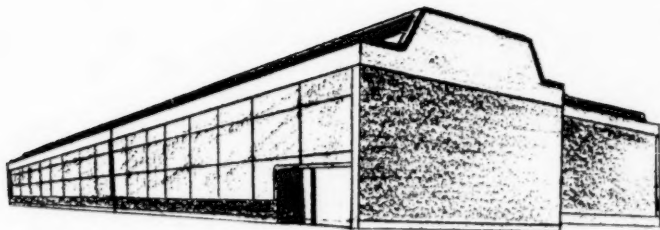
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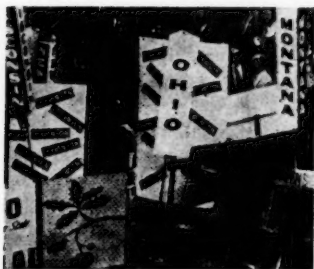
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